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THE

COLLECTED WORKS

OF

SIR HUMPHRY DAVY, Bart.

THE

COLLECTED WORKS

OF

SIR HUMPHRY DAVY, BART.

LL.D. F.R.S.

FOREIGN ASSOCIATE OF THE INSTITUTE OF FRANCE, ETC.

EDITED BY HIS BROTHER,

JOHN DAVY, M.D. F.R.S.

VOL. V.

BAKERIAN LECTURES AND MISCELLANEOUS
PAPERS FROM 1806 TO 1815.

LONDON:
SMITH, ELDER AND CO. CORNHILL.
1840.

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FROM

1806 to 1815.

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1840.

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[This and the succeeding volume contain the Author's mature researches, and his most important discoveries, arranged nearly in the order in which they appeared.

The period of them, comprising nearly twenty-two years, may be divided into two portions; one terminating with his first Continental journey, from which he returned in 1815; the other commencing from that time. The labours of the one are collected in this volume; those of the other will be given in the subsequent volume. The former are principally electro-chemical and chemical; the latter relate more to the operations of nature and the uses of man.

Had he lived to have carried into effect the plan meditated, of revising his labours, especially his analytical ones, and of publishing them in a collected form, it cannot be doubted that he would have altered some parts and have omitted others, which were either not confirmed, or were proved to be erroneous by after research. But such a liberty is manifestly incompatible with the duty of an editor: and, were it not so, it appears to me very questionable if the interests of science would be consulted by omitting even what is proved to be erroneous. The mistakes connected with original inquiry are always more or less instructive; and if properly viewed, may afford hints to other inquirers, or at least inspire courage to persist in the honest search after truth, and not be stopped by failure in one or two instances, or even be disheartened by mistake, which, of all the trials to which the intellect can be subjected, is perhaps the severest.]

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BAKERIAN LECTURES,

&c. &c.

I.

ON SOME CHEMICAL AGENCIES OF ELECTRICITY.*

I. *Introduction.*

THE chemical effects produced by electricity have been for some time objects of philosophical attention; but the novelty of the phenomena, their want of analogy to known facts, and the apparent discordance of some of the results, have involved the inquiry in much obscurity.

An attempt to elucidate the subject will not, I hope, be considered by the Society as unfitted to the design of the Bakerian Lecture. I shall have to detail some minute (and I fear tedious) experiments; but they were absolutely essential to the investigation. I shall likewise, however, be able to offer some illustrations of appearances which hitherto have not been fully explained, and to point out some new properties of one of the most powerful and general of material agents.

---

* [From Philosophical Transactions for 1807; read before the Royal Society, November 20, 1806.]

II. *On the Changes produced by Electricity in Water.*

The appearance of acid and alkaline matter in water acted on by a current of electricity, at the opposite electrified metallic surfaces was observed in the first chemical experiments made with the column of Volta.*

Mr. Cruickshank† supposed that the acid was the nitrous acid and the alkali ammonia. M. Desormes‡ soon after attempted to show by experiments, that muriatic acid and ammonia were the products, and M. Brugnatelli§ asserted the formation of a new and peculiar substance, which he has thought proper to call the electric acid. The experiments said to be made in Italy, and in this country, on the production of muriate of soda are recent,|| and the discussions with regard to them still alive. As early as 1800, I had found that when separate portions of distilled water, filling two glass tubes connected by moist bladders, or any moist animal or vegetable substances, were submitted to the electrical action of the pile of Volta by means of gold wires, a nitro-muriatic solution of gold appeared in the tube containing the positive wire, or the wire transmitting the electricity, and a solution of soda in the opposite tube;¶ but I soon ascertained that the muriatic acid owed its appearance to the animal or vegetable matters employed; for when the same fibres of cotton were made use of in successive experiments, and washed after every process in a weak solution of nitric

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* Nicholson's Journal, 4to. vol. iv. p. 183.

† Ibid. vol. iv. p. 261.

‡ Annales de Chimie, tom. xxxvii. p. 233.

§ Phil. Mag. vol. ix. p. 181.

|| By M. Pacchioni, and by Mr. Peele, Phil. Mag. vol. xxi. p. 279.

¶ I shewed the results of the experiment to Dr. Beddoes at this time; and mentioned the circumstance to Sir James Hall, Mr. Clayfield, and other friends in 1801.

acid, the water in the apparatus containing them, though acted on for a great length of time with a very strong power, at last produced no effect upon solution of nitrate of silver.

In cases when I had procured much soda, the glass at its point of contact with the wire seemed considerably corroded; and I was confirmed in my idea of referring the production of the alkali principally to this source, by finding that no fixed saline matter could be obtained, by electrifying distilled water in a single agate cup from two points of platina connected with the Voltaic battery. Similar conclusions with regard to the appearance of the muriatic acid had been formed by the Galvanic Society of Paris, by Dr. Wollaston, who hit upon the happy expedient of connecting the tubes together by well washed asbestus; and by MM. Biot and Thenard.*

Mr. Sylvester, however, in a Paper published in Mr. Nicholson's Journal for last August, states, that though no fixed alkali or muriatic acid appears when a single vessel is employed; yet that they are both formed when two vessels are used. And to do away all objections with regard to vegetable substances or glass, he conducted his process in a vessel made of baked tobacco-pipe clay inserted in a crucible of platina. I have no doubt of the correctness of his results: but the conclusion appears objectionable. He conceives that he obtained fixed alkali, because the fluid after being heated and evaporated left a matter that tinged turmeric brown, which would have happened had it been lime, a substance that exists in considerable quantities in all pipe-clay; and even allowing the presence of fixed alkali, the materials employed for the manufacture of tobacco-pipes are not at all such as to exclude the combinations of this substance.

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* No. XL. Du Moniteur, 1806. [For many years this Paper was the Journal of Science as well as of War, presenting an incongruous mixture not uncharacteristic of the individual of whom it was the organ. During this period, so little intercourse was there between France and England, that months often elapsed between the publication of a scientific discovery in one country and its being known in the other. And, perhaps, owing to this state connected with war, less delicacy was observed by men of science in the two countries, in engaging in researches which they had not themselves originated, thereby producing an active competition, which on the whole probably was for the advantage of science.]

I resumed the inquiry; I procured small cylindrical cups of agate, of the capacity of about ¼ of a cubic inch each. They were boiled for some hours in distilled water, and a piece of very white and transparent amianthus that had been treated in the same way was made to connect them together; they were filled with distilled water, and exposed by means of two platina wires to a current of electricity, from 150 pairs of plates of copper and zinc 4 inches square, made active by means of solution of alum. After 48 hours the process was examined: paper tinged with litmus plunged into the tube containing the transmitting or positive wire was immediately strongly reddened. Paper coloured by turmeric introduced into the other tube had its colour much deepened; the acid matter gave a very slight degree of turbidness to solution of nitrate of silver. The fluid that affected turmeric retained this property after being strongly boiled; and it appeared more vivid as the quantity became reduced by evaporation; carbonate of ammonia was mixed with it, and the whole dried and exposed to a strong heat: a minute quantity of white matter remained, which, as far as my examination could go, had the properties of carbonate of soda. I compared it with similar minute portions of the pure carbonates of potash and soda. It was not so deliquescent as the former of these bodies, and it formed a salt with nitric acid, which like nitrate of soda soon attracted moisture from a damp atmosphere, and became fluid.

This result was unexpected, but it was far from convincing me that the substances which I had obtained were *generated*. In a similar process, with glass tubes, carried on exactly under the same circumstances, and for the same time, I obtained a quantity of alkali which must have been more than twenty times greater, but no traces of muriatic acid. There was much probability that the agate might contain some minute portion of saline matter, not easily detected by chemical analysis, either in combination, or intimate adhesion in its pores. To determine this, I repeated the experiment a second, a third, and a fourth time. In the second experiment turbidness was still produced by solution of nitrate of silver in the tube containing the acid, but it was less distinct; in the third process it was barely perceptible: and in the fourth the two fluids remained perfectly clear after the mixture. The quantity of alkaline matter diminished in every operation; and in the last process, though the battery had been kept in great activity for three days, the fluid possessed in a very slight degree only the power of acting on paper tinged with turmeric; but its alkaline property was very sensible to litmus paper slightly reddened, which is a much more delicate test: and after evaporation and the process by carbonate of ammonia, a barely perceptible quantity of fixed alkali was still left. The acid matter in the other tube was abundant; its taste was sour; it smelt like water over which large quantities of nitrous gas have been long kept; it did not affect solution of muriate of barytes; and a drop of it placed upon a polished plate of silver left after evaporation a black stain, precisely similar to that produced by extremely diluted nitrous acid.

After these results, I could no longer doubt that some saline matter existing in the agate tubes had been the source of the acid matter capable of precipitating nitrate of silver, and of much of the alkali. Four additional repetitions of the process, however, convinced me that there was likewise some other cause for the presence of this last substance; for it continued to appear to the last, in quantities sufficiently distinguishable, and apparently equal in every case. I had used every precaution; I had included the tubes in glass vessels out of the reach of the circulating air; all the acting materials had been repeatedly washed with distilled water; and no part of them in contact with the fluid had been touched by the fingers.

The only substance which I could now conceive capable of furnishing the fixed alkali was the *water itself.* This water appeared pure by the tests of nitrate of silver and muriate of barytes; but potash and soda, as is well known, rise in small quantities in rapid distillations; and the New River water, which I made use of, contains animal and vegetable impurities, which it was easy to conceive might furnish neutral salts capable of being carried over in vivid ebullition.

To make the experiment in as refined a form as possible, I procured two hollow cones of pure gold containing about 25 grains of water each; they were filled with distilled water, connected together by a moistened piece of amianthus which had been used in the former experiments, and exposed to the action of a Voltaic battery of 100 pairs of plates of copper and zinc of 6 inches square, in which the fluid was a solution of alum and diluted sulphuric acid. In ten minutes the water in the negative tube had gained the power of giving a slight blue tint to litmus paper: and the water in the positive tube rendered it red. The process was continued for 14 hours; the acid increased in quantity during the whole time, and the water became at last very sour to the taste. The alkaline properties of the fluid in the other tube, on the contrary, remained stationary, and at the end of the time, it did not act upon litmus or turmeric paper more than in the first trial: the effect was less vivid after it had been strongly heated for a minute; but evaporation and the usual process proved that *some* fixed alkali was present. The acid; as far as its properties were examined, agreed with pure nitrous acid, having an excess of nitrous gas.

I *repeated* the experiment, and carried on the process for three days; at the end of which time the water in the tube was decomposed and evaporated to more than one-half of its original quantity; the acid was strong, but the alkali in as minute a portion as in the last experiment. It acted indeed rather more vividly on the tests, on account of the greater diminution of the fluid, but presented the same results after being heated.

It was now impossible to doubt that the water contained some substance in very minute quantities, capable of causing the appearance of fixed alkali, but which was soon exhausted; and the question that immediately presented itself was, Is this substance saline matter carried over in distillation? or is it nitrogen gas which exists in minute portions in all water that has been exposed to air, and which, if an element of the fixed alkali, would under the circumstances of the experiment have been soon exhausted, whilst its absorption from the atmosphere would be impeded by the saturation of the water with hydrogen?

I was much more inclined to the former than to the latter supposition. I evaporated a quart of the distilled water that I had used, very slowly at a heat below 140° Fahrenheit, in a silver still; a solid matter remained, equal to $\frac{7}{10}$ of a grain; this matter had a saline but metallic taste, and was deliquescent when exposed to air: I could not obtain from it regular crystals; it did not affect turmeric or litmus, but a part of it, after being heated red, in a silver crucible, exhibited strong alkaline properties. It was not possible to make a minute analysis of so small a quantity, but it appeared to me to be principally a mixture of nitrate of soda and nitrate of lead; and the metallic substance, it is most likely, was furnished by the condensing tube of the common still.

The existence of saline matter in the distilled water being thus distinct, it was easy to determine its operation in the experiment. I filled the two gold cones with water in the usual manner; that negatively electrified, soon attained the maximum of its effect upon turmeric paper. I then introduced into it a very minute portion of the substance obtained by the process of evaporation that has been just described; in less than two minutes its effects were evident; and in five minutes the tint of the paper was changed to a bright brown.

I now conceived that by collecting the water obtained in the second process of slow distillation I should be able to carry on the experiment without *any* appearance of fixed alkali, and the trial proved that I was not mistaken.

Some of this water was introduced into the gold tubes, and the amianthus moistened by it.

After two hours the water in the negative tube produced no effect upon turmeric paper; it did produce an effect upon litmus, which it required great minuteness of observation to perceive; but it wholly lost the power by being heated strongly for two or three minutes, so there is every reason for supposing that it was owing to a small quantity of ammonia.

I made a similar experiment with a portion of the same water in the tubes of agate that had been so often used, and I had the pleasure of finding the results precisely the same.

To detail any more operations of this kind will be unnecessary; all the facts prove that the fixed alkali is not *generated*, but *evolved*, either from the solid materials employed, or from saline matter in the water.

I have made many experiments in vessels composed of different substances, with the water procured by very slow distillation: and in almost every instance some fixed alkali appeared.

In tubes of wax the alkaline matter was a mixture of soda and potash; and the acid matter a mixture of sulphuric, muriatic, and nitric acids.

In a tube of resin, the alkaline matter seemed to be principally potash.

A cube of Carrara marble of about an inch, having an aperture in its centre, was placed in a crucible of platina, which was filled as high as the upper surface of the cube with the purified water, the aperture was filled with the same fluid; the crucible was positively electrified by a strong Voltaic power, and a negatively electrified wire introduced into the aperture.

The water soon gained the property of affecting the tint of turmeric; and fixed alkali and lime were both obtained from it: and this effect took place in repeated experiments: the fixed alkali, however, diminished in quantity every time; and after eleven processes conducted from two to three hours each, disappeared altogether. The production of lime-water was uniform.

I made a solution of 500 grains of this marble in nitric acid; I decomposed the mixture by carbonate of ammonia, and I collected and evaporated the fluid part, and decomposed the nitrate of ammonia by heat. About $\frac{3}{4}$ of a grain of fixed saline matter remained, which had soda for its base.

It was possible that the Carrara marble might have been recently exposed to sea-water; I therefore tried, in the same way, a piece of granular marble, which I had myself broken from a rock on one of the highest of the primitive mountains of Donegal. It afforded fixed alkali by the agency of negative electricity.

A piece of argillaceous schist from Cornwall, treated in the same manner, gave the same result; and serpentine from the Lizard, and grauwacké from North Wales, both afforded soda. It is probable that there are few stones that do not contain some minute portions of saline matter, which in many cases may be mechanically diffused through their substance: and it is not difficult to conceive the possibility of this, when we consider that all our common rocks and strata bear evident marks of having been anciently covered by the sea.

I was now able to determine distinctly, that the soda procured in glass tubes came principally from the glass, as I had always supposed.

I used the two cones of gold with the purified water and the amianthus; the process was conducted as usual. After a quarter of an hour, the negatively electrified tube did not change the colour of turmeric. I introduced into the top of it a bit of glass; in a few minutes the fluid at the surface rendered the tint of the paper of a deep bright brown.

I had never made any experiments, in which acid matter having the properties of nitrous acid was not produced, and the longer the operation the greater was the quantity that appeared.

Volatile alkali likewise seemed to be always formed in very minute portions, during the first few minutes in the purified water in the gold cones, but the limit to its quantity was soon attained.

It was natural to account for both these appearances, from the combination of nascent oxygen and hydrogen respectively with the nitrogen of the common air dissolved in water: and Dr. Priestley's experiments on the absorption of gases by water (on this idea) would furnish an easy explanation of the causes of the constant production of the acid, and the limited production of the alkali: for hydrogen, during its solution in water, seems to expel nitrogen; whilst nitrogen and oxygen are capable of co-existing dissolved in that fluid.*

To render the investigation more complete, I introduced the two cones of gold with purified water under the receiver of an air-pump; the receiver was exhausted till it contained only $\frac{1}{64}$ of the original quantity of air; and then, by means of a convenient apparatus, the tubes were connected with an active Voltaic pile of 50 pairs of plates of 4 inches square. The process was carried on for 18 hours, when the result was examined. The water in the negative tube produced no effect upon prepared litmus, but that in the positive tube gave it a barely perceptible tinge of red.

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* Priestley's Experiments and Observations, vol. i. page 59.

An incomparably greater quantity of acid would have been formed in a similar time in the atmosphere, and the small portion of nitrogen gas remaining in contact with the water, seemed adequate to the effect.

I repeated the experiment under more conclusive circumstances. I arranged the apparatus as before; I exhausted the receiver, and filled it with hydrogen gas from a convenient airholder; I made a second exhaustion, and again introduced hydrogen that had been carefully prepared. The process was conducted for 24 hours, and at the end of this time neither of the portions of the water altered in the slightest degree the tint of litmus.

It seems evident then that water, chemically pure, is decomposed by electricity into gaseous matter alone, into oxygen and hydrogen.

The cause of its decomposition, and of the other decompositions which have been mentioned, will be hereafter discussed.

III. *On the Agencies of Electricity in the Decomposition of various Compounds.*

The experiments that have been detailed on the production of alkali from glass, and on the decomposition of various saline compounds contained in animal and vegetable substances, offered some curious objects of inquiry.

It was evident, that in all changes in which acid and alkaline matter had been present, the acid matter collected in the water round the positively electrified metallic surface; and the alkaline matter round the negatively metallic surface; and this principle of action appeared immediately related to one of the first phenomena observed in the Voltaic pile, the decomposition of the muriate of soda attached to the paste-board; and to many facts which have been since observed on the separation of the constituent parts of neutrosaline and metallic solutions, particularly those detailed by MM. Hisinger and Berzelius.*

The first experiments that I made immediately with respect to this subject were on the decomposition of solid bodies, insoluble, or difficultly soluble in water. From the effects of the electrical agency on glass, I expected that various earthy compounds would undergo change under similar circumstances; and the results of the trials were decided and satisfactory.

Two cups made of compact sulphate of lime, containing about 14 grain-measures of water each, were connected together by fibrous sulphate of lime, which was moistened by pure water: the cups were filled with this fluid; platina wires from the Voltaic battery of 100 pairs of plates of six inches were introduced into them, so that the circuit of electricity was through the fibrous sulphate of lime. In five minutes the water in the cup connected with the positive wire became acid; that in the opposite cup strongly tinged turmeric. After an hour the fluids were accurately examined: when it was found that a pure and saturated solution of lime had been produced in the cup containing the negative wire, which was partially covered with a crust of lime; and that the other cup was filled with a moderately strong solution of sulphuric acid.

I procured two cubical pieces of crystallized sulphate of strontites, of about an inch; a hole was drilled in each capable of containing about 8 grains of water: the cubes were plunged in pure water in a platina crucible;

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* Annales de Chimie, tom. li. p. 167.

and the level of the fluid preserved a few lines below the surface of the cubes; two platina wires were introduced into the holes, which were filled with pure water. The disengagement of gas, when the wires were connected with the battery of 100, proved that the sulphate of strontites was sufficiently porous to form a proper conducting chain. The results were much longer in being obtained in this experiment than in the last: some time elapsed before a sensible effect could be perceived; but the termination was similar. In 30 hours the fluid in the cavity containing the negative wire had gained the property of precipitating solution of sulphate of potash; and the presence of sulphuric acid in the other cavity was evident from its effect upon solution of muriate of barytes.

I made an experiment upon fluate of lime under like circumstances; but the crystallized fluate not being equally permeable to moisture, the two cavities were connected by moist asbestus. This decomposition was likewise very slow; but in the course of two days a pretty strong solution of lime was obtained in one tube; and an acid fluid in the other, which precipitated acetite of lead, and left a spot upon the glass from which it had evaporated.

Sulphate of barytes, as might be supposed, proved much more difficult of decomposition than either sulphate of strontites or fluate of lime. I had made four or five experiments upon it, with the same kind of apparatus that had been applied to the fluate of lime, before I was able to gain decided results. In the last process performed on this substance, two pieces of a large single crystal were hollowed by grinding, so as to contain about five grains of water each; they were connected by moist asbestus, and constantly subjected during four days to the strong action of a battery of 150 pairs of plates of 4 inches square. As the water diminished, its place was supplied by new quantities. At the conclusion of the experiment the fluid on the positive side of the apparatus instantly reddened litmus, tasted very sour, and gave a distinct precipitate with a solution of muriate of barytes; the water on the other side deepened the tincture of turmeric; but did not render solution of sulphate of potash turbid. There was a small quantity of white crust, however, on the sides and the bottom of the cavity, and I conceived that this might be the barytes, which, during the extremely slow decomposition, would have combined with the carbonic acid of the atmosphere. To ascertain if this had been the case, I introduced into the cavity a drop of diluted muriatic acid; a slight effervescence appeared, and the fluid obtained occasioned a distinct white cloudiness in solution of sulphate of soda.

In all these cases the constituent parts of the bodies newly arranged by the effects of electricity existed in considerable quantities, and exposed on a large surface to its action. I had great reason to believe, however, from the trials with distilled water in different vessels, that very minute portions of acid and alkaline matter might be disengaged by this agency from solid combinations, principally consisting of the pure earths.

This part of the investigation was easily elucidated.

For a purpose of geological inquiry, which on a future occasion I shall have the honour of laying before the Society, I had made a careful analysis of a specimen of fine grained basalt from Port Rush in the county of Antrim, by means of fusion with boracic acid: it had afforded in 100 parts 3½ parts of soda, and nearly ½ a part of muriatic acid, with 15 parts of lime. This stone appeared to me very well fitted for the purpose of experiment: cavities were drilled in two pieces, properly shaped; they contained about 12 grains of water each; they were connected by moistened amianthus, and the process conducted as usual with a power of 50 pairs of plates. At the end of ten hours the result was examined with care. The fluid that had been positively electrified had the strong smell of oxymuriatic acid, and copiously precipitated nitrate of silver; the other portion of fluid affected turmeric, and left by evaporation a substance which seemed to be a mixture of lime and soda.

A part of a specimen of compact zeolite, from the Giant's Causeway, which by analysis had given 7 parts in 100 of soda, had a small cavity made in it; it was immerged in pure water in a crucible of platina, and electrified in the same manner as the cube of Carrara marble, mentioned in page 9. In less than two minutes the water in the cavity had gained the property of changing the colour of turmeric; and in half an hour the solution was disagreeably alkaline to the taste. The matter dissolved proved to be soda and lime.

Lepidolite, treated in the same way, gave potash.

A piece of vitreous lava, from Etna, gave alkaline matter, which seemed to be a mixture of soda, potash, and lime.

As in these trials the object was merely to ascertain the general fact of decomposition, the process was never conducted for a sufficient time to develope a quantity of alkaline matter capable of being conveniently weighed, and of course any loss of weight of the substance could not be determined.

I thought it right, however, to make one experiment of this kind, for the sake of removing every possibility of doubt on the source of the different products; and I selected for this purpose glass, as a substance apparently insoluble in water, and not likely to afford in any way erroneous results.

The balance that I employed was made for the Royal Institution, by Mr. Fidler, after the model of that belonging to the Royal Society; it turns readily with $\frac{1}{500}$ of a grain when loaded with 100 grains on each side; a glass tube with a platina wire attached, weighing 84 grains $\frac{56}{128}$ was connected with an agate cup, by amianthus; they were filled with purified water, and electrified by a power from 150 pairs of plates, in such a way that the platina in the glass tube was negative. The process was continued for 4 days, when the water was found alkaline. It gave by evaporation and exposure to a heat of about 400° Fahrenheit, soda mixed with a white powder insoluble in acids, the whole weight of which $\frac{36}{128}$ of a grain. The glass tube carefully cleaned and dried weighed 84 grains, $\frac{37}{128}$. The difference between the loss of weight of the tube and the weight of the products in the water may be easily explained: some minute detached particles of amianthus were present, and the soda must have contained water, a substance which it is probably perfectly free from in glass.

Having obtained such results with regard to the disengagements of the saline parts of bodies insoluble in water, I made a number of experiments on soluble compounds; their decomposition was always much more rapid, and the phenomena perfectly distinct.

In these processes I employed the agate cups with platina wires, connected by amianthus moistened in pure water; the solutions were introduced into the cups, and the electrifying power applied from batteries of 50 pairs of plates, in the usual way.

A diluted solution of sulphate of potash treated in this manner, produced in four hours at the negative wire a weak lixivium of potash; and a solution of sulphuric acid at the positive wire.

The phenomena were similar when sulphate of soda, nitrate of potash, nitrate of barytes, sulphate of ammonia, phosphate of soda, succinate, oxalate, and benzoate of ammonia, and alum were used. The acids in a certain time collected in the tube containing the positive wire, and the alkalies and earths in that containing the negative wire.

Solutions of the muriatic salts, decomposed in the same way, uniformly gave oxymuriatic acid on the positive side.

When compatible mixtures of neutrosaline solutions containing the common mineral acids were used, the different acids and the different bases seemed to separate together in a mixed state, without any respect to the orders of affinity.

When metallic solutions were employed, metallic crystals or depositions were formed, as in common galvanic experiments, on the negative wire, and oxide was likewise deposited round it; and a great excess of acid was soon found in the opposite cup. With solutions of iron, zinc, and tin, this effect took place, as well as with the more oxidable metals: when muriate of iron was used, the black substance deposited upon the wire was magnetic, and dissolved with effervescence in muriatic acid; and when sulphate of zinc was used, a gray powder possessed of the metallic lustre, and likewise soluble with effervescence, appeared; and in all cases acid in excess was exhibited on the positive side.

Strong or saturated saline solutions, as might have been expected, afforded indications of the progress of decomposition much more rapidly than weak ones; but the smallest proportion of neutrosaline matter seemed to be acted on with energy.

A very simple experiment demonstrates this last principle. If a piece of paper tinged with turmeric, is plunged into pure water in a proper circuit, in contact with the negative point, the very minute quantity of saline compound contained in the paper, affords alkaline matter sufficient to give it instantly a brown tint near its point of contact: and acid in the same manner is immediately developed from litmus paper, at the positive surface.

I made several experiments, with the view of ascertaining whether, in the decompositions by electricity, the separation of the constituent parts was complete from the last portions of the compound; and whenever the results were distinct, this evidently appeared to be the case.

I shall describe one of the most conclusive of the experiments: a very weak solution of sulphate of potash, containing 20 parts water and one part saturated solution, at 64°, was electrified in the two agate cups by the power of 50 pairs of plates for three days: the connecting amianthus, which had been moistened with pure water, was removed, washed with pure water, and again applied, twice every day; by this precaution the presence of any neutral salt that might adhere to it, and disturb the results was prevented. The alkali obtained in this process in the solution had the properties of pure potash; and when it had been saturated with nitric acid it gave no turbidness by mixture with solution of muriate of barytes: the acid matter exposed to a strong heat evaporated without leaving any residuum.

IV. *On the Transfer of certain of the constituent Parts of Bodies by the Action of Electricity.*

M. Gautherot has stated,* that in a single galvanic circle of zinc, silver, and water, in an active state, the oxide of zinc formed is attracted by the silver; † and MM. Hisinger and Berzelius detail an account of an experiment, in which solution of muriate of lime being placed in the positive part of a siphon, electrified by wires from a Voltaic pile, and distilled water in the negative part, lime appeared in the distilled water.

These facts rendered it probable, that the saline elements evolved in decompositions by electricity were capable of being transferred from one electrified surface to another, according to their usual order of arrangement: but to demonstrate this clearly, new researches were wanting.

I connected one of the cups of sulphate of lime, mentioned page 13, with a cup of agate by asbestus; and, filling them with purified water, made the platina wire in the cup of sulphate of lime transmit the electricity from a power of 100; a wire in the agate cup received it. In about four hours a strong solution of lime was found in the agate cup, and sulphuric acid in the cup of sulphate of lime. By reversing the order, and carrying on the process for a similar time, the sulphuric acid appeared in the agate cup, and the solution of lime on the opposite side.

Many trials were made with other saline substances, with analogous results. When the compounds of the strong mineral acids with alkaline or alkaline-earthy bases, were introduced into one tube of glass, distilled water connected by amianthus being in another tube, both connected by wires of platina in the Voltaic arrangement, the base always passed into the distilled water when it was negative, and the acid when it was positive.

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* Annales de Chimie, vol. xxxix. page 203.

† Ibid. vol. li. page 171.

The metals and the metallic oxides passed towards the negative surface like the alkalies, and collected round it. In a case in which solution of nitrate of silver was used on the positive side, and distilled water on the negative, silver appeared on the whole of the transmitting amianthus, so as to cover it with a thin metallic film.

The time required for these transmissions (the quantity and intensity of the electricity, and other circumstances remaining the same) seemed to be in some proportion as the length of the intermediate volume of water. Thus when with the power of 100, sulphate of potash was on the negative side, and distilled water on the positive side, the distance between the wires being only an inch, sulphuric acid, in sufficient quantity to be very manifest, was found in the water in less than five minutes: but when the tubes were connected by an intermediate vessel of pure water, so as to make the circuit 8 inches, 14 hours were required to produce the same effect.

To ascertain whether the contact of the saline solution with a metallic surface was necessary for the decomposition and transfer, I introduced purified water into two glass tubes; a vessel containing solution of muriate of potash was connected with them respectively by amianthus; and the arrangement was made in such a way, that the level of both the portions of purified water was higher than the level of the saline solution.

In this case, the saline matter was distant from each of the wires at least $\frac{2}{3}$ of an inch; yet alkaline matter soon appeared in one tube, and acid matter in the other: and in 16 hours moderately strong solutions of potash, and of muriatic acid had been formed.

In this case of electrical transfer or attraction, the acid and alkaline matter seemed to be perfectly pure; and I am inclined to believe that this is uniformly the case in all experiments carefully made. One of the instances in which I conceived acid most likely to be present, was in the transfer of magnesia from sulphate of magnesia in the positive tube, to distilled water in the negative tube. I examined the case, taking care that the distilled water was never upon a lower level than the saline solution: the process was continued for some hours, till a considerable quantity of magnesia had appeared. The connecting amianthus was removed, and muriatic acid poured into the tube: the saturated solution did not precipitate solution of muriate of barytes.

I endeavoured to ascertain the progress of the transfer, and the course of the acid or alkaline matter in these decompositions, by using solutions of litmus and turmeric, and papers coloured by these substances: and these trials led to the knowledge of some singular and unexpected circumstances.

Two tubes, one containing distilled water, the other solution of sulphate of potash, were each connected by amianthus with a small ounce measure filled with distilled water tinged by litmus: the saline solution was negatively electrified; and as it was natural to suppose, that the sulphuric acid in passing through the water to the positive side would redden the litmus in its course, some slips of moistened paper tinged with litmus were placed above and below the pieces of amianthus, directly in the circuit. The progress of the experiment was minutely observed; the first effect of reddening took place immediately above the positive surface, where I had least expected it; the red tint slowly diffused itself from the positive side to the middle of the vessel, but no redness appeared above the amianthus, or about it, on the negative side, and though it had been constantly transmitting sulphuric acid, it remained unaffected to the last.

The order of the experiment was changed, and the saline solution placed on the positive side; a solution and papers tinged with turmeric being substituted for those tinged with litmus. The effect was precisely analogous; the turmeric became brown first, near the negative wire, and no change took place in the intermediate vessel near the positive wire.

In another process, the two glass tubes were filled with solution of muriate of soda, and the intermediate vessel with solution of sulphate of silver; paper tinged with turmeric was placed on the positive side, and paper tinged with litmus on the negative side; as soon as the electrical circuit was complete, soda began to appear in the negative tube, and oxymuriatic acid in the positive tube; and the alternate products were exhibited passing into the solution of sulphate of silver, the muriatic acid occasioning a dense heavy precipitate, and the soda a more diffused and a lighter one; but neither the turmeric transmitting the alkali, nor the litmus transmitting the acid, had their tints in the slightest degree altered.

V. *On the Passage of Acids, Alkalies, and other Substances through various attracting chemical Menstrua, by means of Electricity.*

As acid and alkaline substances during the time of their electrical transfer passed through water containing vegetable colours without affecting them, or apparently combining with them, it immediately became an object of inquiry, whether they would not likewise pass through chemical menstrua, having stronger attractions for them; and it seemed reasonable to suppose, that the same power which destroyed elective affinity in the vicinity of the metallic points, would likewise destroy it, or suspend its operation, throughout the whole of the circuit.

An arrangement was made, of the same vessels and apparatus employed in the experiment on the solution of muriate of soda and sulphate of silver page 23. Solution of sulphate of potash was placed in contact with the negatively electrified point, pure water was placed in contact with the positively electrified point, and a weak solution of ammonia was made the middle link of the conducting chain; so that no sulphuric acid could pass to the positive point in the distilled water, without passing through the solution of ammonia.

The power of 150 was used: in less than five minutes it was found, by means of litmus paper, that acid was collecting round the positive point; in half an hour, the result was sufficiently distinct for accurate examination.

The water was sour to the taste, and precipitated solution of nitrate of barytes.

Similar experiments were made with solution of lime; and weak solutions of potash and soda, and the results were analogous. With strong solutions of potash and soda a much longer time was required for the exhibition of the acid; but even with the most saturated alkaline lixivium, it always appeared in a certain period.

Muriatic acid, from muriate of soda, and nitric acid from nitrate of potash, were transmitted through concentrated alkaline menstrua, under similar circumstances.

When distilled water was placed in the negative part of the circuit, and a solution of sulphuric, muriatic, or nitric acid, in the middle, and any neutral salt with a base of lime, soda, potash, ammonia, or magnesia, in the positive part, the alkaline matter was transmitted through the acid matter to the negative surface, with similar circumstances to those occurring during the passage of the acid through the alkaline menstrua; and the less concentrated the solution, the greater seemed to be the facility of transmission.

I tried in this way muriate of lime with sulphuric acid, nitrate of potash with muriatic acid, sulphate of soda with muriatic acid, and muriate of magnesia with sulphuric acid; I employed the power of 150; and in less than 48 hours, I gained in all these cases decided results; and magnesia came over like the rest.

Strontites and barytes passed, like the other alkaline substances, readily through muriatic and nitric acids; and, *vice versâ*, these acids passed with facility through aqueous solutions of barytes and strontites; but in experiments in which it was attempted to pass sulphuric acid through the *same menstrua*, or to pass barytes or strontites through this acid, the results were very different.

When solution of sulphate of potash was in the negative part of the circuit, distilled water in the positive part, and saturated solution of barytes in the middle, no sensible quantity of sulphuric acid existed in the distilled water after 30 hours, the power of 150 being used; after four days, sulphuric acid appeared, but the quantity was extremely minute; much sulphate of barytes had formed in the intermediate vessel; the solution of barytes was so weak as barely to tinge litmus; and a thick film of carbonate of barytes had formed on the surface of the fluid. With solution of strontites the result was very analogous, but the sulphuric acid was sensible in three days.

When solution of muriate of barytes was made positive by the power of 150, concentrated sulphuric acid intermediate, and distilled water negative, no barytes appeared in the distilled water, when the experiment had been carried on for four days; but much oxymuriatic acid had formed in the positive vessel, and much sulphate of barytes had been deposited in the sulphuric acid.

Such of the metallic oxides as were made subjects of experiment passed through acid solutions from the positive to the negative side, but the effect was much longer in taking place than in the instances of the transition of alkaline matter. When solution of green sulphate of iron was made positive, solution of muriatic acid intermediate, and water negative, in the usual arrangement, green oxide of iron began to appear in about ten hours upon the negative connecting amianthus, and in three days a considerable portion had been deposited in the tube. Analogous results were obtained with sulphate of copper, nitrate of lead, and nitromuriate of tin.

I made several experiments on the transition of alkaline and acid matter through different neutrosaline solutions, and the results were such as might well have been anticipated.

When solution of muriate of barytes was negative, solution of sulphate of potash intermediate, and pure water positive, the power being from 150, sulphuric acid appeared in about five minutes in the distilled water; and in two hours the muriatic acid was likewise very evident. When solution of sulphate of potash was positive, solution of muriate of barytes intermediate, and distilled water negative, the barytes appeared in the water in a few minutes; the potash from the more remote part of the chain was nearly an hour in accumulating, so as to be sensible.

When the solution of muriate of barytes was positive, the solution of sulphate of potash intermediate, and distilled water negative, the potash soon appeared in the distilled water; a copious precipitation of sulphate of barytes formed in the middle vessel; but after ten hours no barytes had passed into the water.

When solution of sulphate of silver was interposed between solution of muriate of barytes on the negative side, and pure water on the positive side, sulphuric acid alone passed into the distilled water; and there was a copious precipitation in the solution of sulphate of silver. This process was carried on for ten hours.

I tried several of these experiments of transition upon vegetable and animal substances with perfect success.

The saline matter exposed in contact with the metal, and that existing in the vegetable or animal substances, both underwent decomposition and transfer; and the time of the appearance of the different products at the extremities of the circuit was governed by the degree of their vicinity.

Thus, when a fresh leaf-stalk of the polyanthus, about two inches long, was made to connect a positively electrified tube containing solution of nitrate of strontites, and a negatively electrified tube containing pure water, the water soon became green, and gave indications of alkaline properties, and free nitric acid was rapidly separated in the positive tube. After ten minutes, the alkaline matter was examined; it consisted of potash and lime, and as yet no strontites had been carried into it; for the precipitate it gave with sulphuric acid readily dissolved in muriatic acid. In half an hour strontites, however, appeared; and in four hours it formed a very abundant ingredient of the solution.

A piece of muscular flesh of beef, of about three inches in length and half an inch in thickness, was treated in the same way as the medium of communication between muriate of barytes and distilled water. The first products were soda, ammonia, and lime; and after an hour and a quarter, the barytes was very evident. There was much free oxymuriatic acid in the positively electrified tube, but no particle of muriatic acid had passed into the negative tube, either from the muriatic solution or from the muscular fibre.

VI. *Some general Observations on these Phenomena, and on the Mode of Decomposition and Transition.*

It will be a general expression of the facts that have been detailed, relating to the changes and transitions by electricity, in common philosophical language, to say, that hydrogen, the alkaline substances, the metals, and certain metallic oxides, are attracted by negatively electrified metallic surfaces, and repelled by positively electrified metallic surfaces; and contrariwise, that oxygen and acid substances are attracted by positively electrified metallic surfaces, and repelled by negatively electrified metallic surfaces; and these attractive and repulsive forces are sufficiently energetic to destroy or suspend the usual operation of elective affinity.

It is very natural to suppose, that the repellent and attractive energies are communicated from one *particle to another particle* of the same kind, so as to establish a conducting chain in the fluid; and that the locomotion takes place in consequence; and that this is really the case seems to be shown by many facts. Thus, in all the instances in which I examined alkaline solutions through which acids had been transmitted, I always found acid in them whenever any acid matter remained at the original source. In time, by the attractive power of the positive surface, the decomposition and transfer undoubtedly become complete; but this does not affect the conclusion.

In the cases of the separation of the constituents of water, and of solutions of neutral salts forming the whole of the chain, there may possibly be a succession of decompositions and recompositions throughout the fluid. And this idea is strengthened by the experiments on the attempt to pass barytes through sulphuric acid, and muriatic acid through solution of sulphate of silver, in which, as insoluble compounds are formed and carried out of the sphere of the electrical action, the power of transfer is destroyed. A similar conclusion might likewise be drawn from many other instances. Magnesia and the metallic oxides, as I have already mentioned, will pass along moist amianthus from the positive to the negative surface; but if a vessel of pure water be interposed, they do not reach the negative vessel, but sink to the bottom. These experiments I have very often made, and the results are perfectly conclusive; and in the case, page 26, in which sulphuric acid seemed to pass in small quantities through very weak solutions of strontites and barytes, I have no doubt but that it was carried through by means of a thin stratum of pure water, where the solution had been decomposed at the surface by carbonic acid; for in an experiment similar to these in which the film of carbonate of barytes was often removed and the fluid agitated, no particle of sulphuric acid appeared in the positive part of the chain.

It is easy to explain, from the general phenomena of decomposition and transfer, the mode in which *oxygen* and *hydrogen* are separately evolved from water. The oxygen of a portion of water is attracted by the positive surface, at the same time that the other constituent part, the hydrogen is repelled by it; and the opposite process takes place at the negative surface; and in the middle or neutral point of the circuit, whether there be a series of decompositions and recompositions, or whether the particles from the extreme points only are active, there must be a new combination of the repelled matter: and the case is analogous to that of two portions of muriate of soda separated by distilled water; muriatic acid is repelled from the negative side, and soda from the positive side, and muriate of soda is composed in the middle vessel.

These facts seem fully to invalidate the conjectures of M. Ritter, and some other philosophers, with regard to the elementary nature of water, and perfectly to confirm the great discovery of Mr. Cavendish.

M. Ritter conceived that he had procured oxygen from water without hydrogen, by making sulphuric acid the medium of communication at the negative surface; but in this case, sulphur is deposited, and the oxygen from the acid, and the hydrogen from the water are respectively repelled; and the new combination produced.

I have attempted some of the experiments of decomposition and transfer, by means of common electricity, making use of a powerful electrical machine of Mr. Nairne's construction, belonging to the Royal Institution, of which the cylinder is fifteen inches in diameter, and two feet long.

With the same apparatus as that employed for decompositions by the Voltaic battery, no perceptible effect was produced by passing a strong current of electricity silently for four hours through solution of sulphate of potash.

But by employing fine platina points of $\frac{1}{70}$ of an inch in diameter, cemented in glass tubes in the manner contrived by Dr. Wollaston,* and bringing them near each other, in vessels containing from three to four grains of the solution, and connected by moist asbestus, potash appeared in less than two hours round the negatively electrified point, and sulphuric acid round the positive point.

In a similar experiment sulphuric acid was transferred through moist asbestus into water; so that there can be no doubt, that the principle of action is the same in common and the Voltaic electricity.†

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* Phil. Trans. vol. xci. p. 427.

† This had been shewn, with regard to the decomposition of water, by Dr. Wollaston's important researches.—By carefully avoiding sparks, I have been able to obtain the two constituents in a separate state. In an experiment in which a fine platina point cemented in glass, and connected by a single wire with the positive conductor of this machine, was plunged in distilled water in an insulated state, and the electricity dissipated into the atmosphere by means of moistened filaments of cotton, oxygen gas, mixed with a little nitrogen gas, was produced; and when the same apparatus was applied to the negative conductor hydrogen gas

VII. *On the general Principles of the Chemical Changes produced by Electricity.*

The experiments of Mr. Bennet had shown, that many bodies brought into contact and afterwards separated, exhibited *opposite* states of electricity; but it is to the investigations of Volta that a clear development of the fact is owing; he has distinctly shown it in the case of copper and zinc, and other metallic combinations; and has supposed that it also takes place with regard to metals and fluids.

In a series of experiments made in 1801,* on the construction of electrical combinations by means of alternations of single metallic plates, and different strata of fluids, I observed that when acid and alkaline solutions were employed as elements of these instruments, the alkaline solutions always received the electricity from the metal, and the acid always transmitted it to the metal; thus, in an arrangement of which the elements were tin, water, and solution of potash, the circulation of the electricity was from the water to the tin, and from the tin to the solution of potash; but in an arrangement composed of weak nitric acid, water, and tin; the order was from the acid to the tin, and from the tin to the water.

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was evolved, and a minute portion of oxygen and nitrogen gases; but neither of the foreign products, the nitrogen gas in the one case and the nitrogen and oxygen gases in the other, formed as much as $\frac{1}{50}$ part of the volume of the gases; and there is every reason to suppose that they were derived from the extrication of common air, which had been dissolved in the water. This result, which when I first obtained it in 1803, appeared very obscure, is now easily explained; the alternate products must have been evolved at the points of the dissipation of the electricity.

* See Phil. Trans. vol. xci. p. 297. [Vol. II. p. 182.]

These principles seem to bear an immediate relation to the general phenomena of decomposition and transference, which have been the subject of the preceding details.

In the simplest case of electrical action, the alkali which receives electricity from the metal would necessarily, on being separated from it, appear positive; whilst the acid under similar circumstances would be negative: and these bodies having respectively with regard to the metals, that which may be called a positive and a negative electrical energy, in their repellent and attractive functions seem to be governed by laws the same as the common laws of electrical attraction and repulsion. The body possessing the positive energy being repelled by positively electrified surfaces, and attracted by negatively electrical surfaces; and the body possessing the negative energy following the contrary order.

I have made a number of experiments with the view of elucidating this idea, and of extending its application; and in all cases they have tended to confirm the analogy in a remarkable manner.

Well burned charcoal, water and nitric acid, the same substance, water and solution of soda, made respectively elements of different electrical combinations, became distinctly active when 20 alternations were put together: the positive energy being exhibited on the side of the alkali, and the negative on that of the acid. Arrangements of plates of zinc, pieces of moistened pasteboard, and moistened quicklime, to the number of 40 series, likewise formed a weak electrical pile, the effect of the lime being similar to that of an alkali, but the power was soon lost.

I endeavoured, by means of very delicate instruments, to ascertain the electrical states of single insulated acid and alkaline solutions, after their contact with metals; and for this purpose I employed at different times the condensing electrometer of Mr. Cuthbertson's construction, Mr. Cavallo's multiplier, and a very sensible electrical balance, on the principle of torsion, adopted by M. Coulomb; but the effects were unsatisfactory, the circumstances of evaporation, and of chemical action, and the adherence of the solutions to the surfaces of the metals employed, in most cases, prevented any distinct result, or rendered the source of the electricity doubtful. I shall not enter into any details of these processes, or attempt to draw conclusions from capricious and uncertain appearances, which, as we shall immediately see, may be fully deduced from clear and distinct ones.

The alkaline and acid substances capable of existing in the dry and solid form, give by contact with the metals exceedingly sensible electricities, which require for their exhibition the gold leaf electrometer only with the small condensing plate.

When oxalic, succinic, benzoic, or boracic acid, perfectly dry, either in powder or crystals, were touched upon an extended surface with a plate of copper insulated by a glass handle, the copper was found positive, the acid negative. In favourable weather, and when the electrometer was in perfect condition, one contact of the metal was sufficient to produce a sensible charge; but seldom more than five or six were required. Other metals, zinc and tin for instance, were tried with the same effect. And the metal received the positive charge, apparently to the same extent, whether the acid was insulated upon glass, or connected with the ground.

The solid acid of phosphorus, which had been strongly ignited, and most carefully excluded from the contact of air, rendered the insulated plate of zinc positive by four contacts; but after exposure to the atmosphere for a few minutes it wholly lost this power.

When metallic plates were made to touch dry lime, strontites, or magnesia, the metal became negative; the effect was exceedingly distinct, a single contact upon a large surface being sufficient to communicate a considerable charge. For these experiments the earths were carefully prepared; they were in powder, and had been kept for several days in glass bottles before they were used: it is essential to the success of the process that they be of the temperature of the atmosphere. In some experiments which I made upon them when cooling, after having been ignited, they appeared strongly electrical, and rendered the conductors brought in contact with them positive.

I made several experiments in a similar manner on the effects of the contact of potash and soda with the metals. Potash in no instance afforded a satisfactory result; its powerful attraction for water presents an obstacle probably unsurmountable to the success of any trials made in the free atmosphere. Soda, in the only case in which electricity was exhibited, affected the metal in the same way as lime, strontites, and magnesia. Upon this occasion the soda had been prepared with great care, exposed in a platina crucible for nearly an hour in a red heat, and suffered to cool in the crucible inverted over mercury; when cool it was immediately removed, and the contact made with a plate of zinc; the experiment was performed in the open air; the weather was peculiarly dry, the thermometer stood at 28° Fahrenheit, and the barometer at 30·2 inches; six contacts gave a charge to the condensing electrometer in the first trial; in the second ten were required to produce a similar effect; and after this, though two minutes only had elapsed, no farther result could be obtained.

In the decomposition of sulphuric acid by Voltaic electricity the sulphur separates on the negative side. The experiments of various electricians prove, that by the friction of sulphur and metals, the sulphur becomes positive and the metals negative; the same thing I find happens from the contact of an unexcited cake of sulphur and insulated metallic plates. Mr. Wilke has stated an exception to lead, as rendering sulphur negative by its friction. The results that I have obtained with lead, in trials very carefully made, are the same as those with other metals.* Sulphur, by being rubbed or struck against newly-polished lead, always became positive. Mr. Wilke perhaps was misled by using tarnished lead: sulphur, I find, rubbed against litharge, or lead the surface of which has been long exposed to air, becomes negative; and this exception being removed, all the facts on the subject are confirmations of the general principle.†

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* As sulphur is a nonconductor, and easily excited by slight friction, or small changes in its temperature, some caution is required in drawing conclusions from the experiments in which it is employed. Sulphur, examined immediately after having been heated, gives a positive charge to conductors, agreeing in this respect with the alkaline substances; and a slight contact with the dry hand is sufficient to render it negative. In general likewise in experiments of contact care should be taken that the metallic plate is free from electricity: well polished plates of copper and zinc will, I find, receive a negative charge from being laid on a table of common mahogany.

† Concentrated solution of phosphoric acid, I find, is decomposed by Voltaic electricity: the phosphorus combines with the negatively electrified metal, and forms a phosphuret; at least this happened in the two cases that I tried with platina and copper. From all analogy it may be inferred, that the electrical energy of this inflammable substance with

On the general principle, oxygen and hydrogen ought to possess, with regard to the metals respectively, the negative and positive energy. This I have not been able to prove by direct experiments of contact; but the idea is confirmed by the agency of their compounds; thus I have found that solution of sulphuretted hydrogen in water acts in the electrical apparatus composed of single plates and different strata of fluids, in the same manner as alkaline solutions; and that solution of oxymuriatic acid is more powerful in similar arrangements than solutions of muriatic acid of a higher degree of concentration; and in both these cases, it is impossible to conceive the combined hydrogen and oxygen inactive. The inference likewise is fully warranted by the case of the solutions of alkaline hydroguretted sulphurets, which consisting principally of alkali and sulphur together in union with water, exhibit the positive energy with regard to the metals in a very high degree. In the series of experiments on Voltaic arrangements constructed with single plates above-mentioned, I found the solutions of hydroguretted sulphurets in general much more active than alkaline solutions, and particularly active with copper, silver, and lead. And in an experiment that I made on a combination of copper, iron, and hydroguretted sulphurets of potash, in 1802, I found that the positive energy of the hydroguretted sulphurets with regard to the copper was sufficient to overpower that of the iron; so that the electricity did not circulate from the copper to the iron, and from the

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regard to metals is the same as that of sulphur. I tried some experiments of contact upon it, but without success. Its slow combustion in the atmosphere it is most likely was the cause of the failure: but even in gases not containing free or loosely combined oxygen, its evaporation would probably interfere.

iron to the fluid, as in common cases, but from the copper to the hydroguretted sulphuret, and from the hydroguretted sulphuret to the iron.

All these details afford the strongest confirmation of the principle. It may be considered almost as a mere arrangement of facts; and with some extensions it seems capable of being generally applied.

Bodies possessing opposite electrical energies with regard to one and the same body, we might fairly conclude would likewise possess them with regard to each other. This I have found by experiment is the case with lime and oxalic acid. A dry piece of lime, made from a very pure compact secondary limestone, and of such a form as to present a large smooth surface, became positively electrical by repeated contacts with crystals of oxalic acid: and these crystals placed upon the top of a condensing electrometer, and repeatedly touched by the lime, which after each contact was freed from its charge, rendered the gold leaves negatively electrical. The tendency of the mere contacts of the acid and alkali with the metal would be to produce opposite effects to those exhibited, so that their mutual agency must have been very energetic.

It will not certainly be a remote analogy to consider the other acid and alkaline substances generally, and oxygen and hydrogen as possessing similar electrical relations; and in the decompositions and changes presented by the effects of electricity, the different bodies naturally possessed of chemical affinities appear incapable of combining, or of remaining in combination, when placed in a state of electricity different from their natural order. Thus, as we have seen, the acids in the positive part of the circuit, separate themselves from alkalies, oxygen from hydrogen, and so on; and metals on the negative side, do not unite to oxygen, and acids do not remain in union with their oxides; and in this way the attractive and repellent agencies seem to be communicated from the metallic surfaces throughout the whole of the menstruum.

VIII. *On the Relations between the Electrical Energies of Bodies, and their Chemical Affinities.*

As the chemical attraction between two bodies seems to be destroyed by giving one of them an electrical state different from that which it naturally possesses; that is, by bringing it artificially into a state similar to the other, so it may be increased by exalting its natural energy. Thus, whilst zinc, one of the most oxidable of the metals, is incapable of combining with oxygen when negatively electrified in the circuit, even by a feeble power; silver, one of the least oxidable, easily unites to it when positively electrified; and the same thing might be said of other metals.

Amongst the substances that combine chemically, all those, the electrical energies of which are well known, exhibit opposite states; thus, copper and zinc, gold and quicksilver, sulphur and the metals, the acid and alkaline substances, afford apposite instances; and supposing perfect freedom of motion in their particles or elementary matter, they ought, according to the principles laid down, to attract each other in consequence of their electrical powers. In the present state of our knowledge, it would be useless to attempt to speculate on the remote cause of the electrical energy, or the reason why different bodies, after being brought into contact, should be found differently electrified; its relation to chemical affinity is, however, sufficiently evident. May it not be identical with it, and an essential property of matter?

The coated glass plates of Beccaria strongly adhere to each other when oppositely charged, and retain their charges on being separated. This fact affords a distinct analogy to the subject; different particles in combining must still be supposed to preserve their peculiar states of energy.

In the present early stage of the investigation, it would be improper to place unbounded confidence in this hypothesis; but it seems naturally to arise from the facts, and to coincide with the laws of affinity, so ably developed by modern chemists; and the general application of it may be easily made.

Supposing two bodies, the particles of which are in different electrical states, and those states sufficiently exalted to give them an attractive force superior to the power of aggregation, a combination would take place which would be more or less intense according as the energies were more or less perfectly balanced; and the change of properties would be correspondently proportional.

This would be the simplest case of chemical union. But different substances have different degrees of the same electrical energy in relation to the same body: thus the different acids and alkalies are possessed of different energies with regard to the same metal; sulphuric acid, for instance, is more powerful with lead than muriatic acid, and solution of potash is more active with tin than solution of soda. Such bodies likewise may be in the same state or repellent with regard to each other, as apparently happens in the cases just mentioned; or they may be neutral; or they may be in opposite or attracting states, which last seems to be the condition of sulphur and alkalies that have the same kind of energy with regard to metals.

When two bodies repellent of each other act upon the same body with different degrees of the same electrical attracting energy, the combination would be determined by the degree; and the substance possessing the weakest energy would be repelled; and this principle would afford an expression of the causes of elective affinity, and the decompositions produced in consequence.

Or where the bodies having different degrees of the same energy, with regard to the third body, had likewise different energies with regard to each other, there might be such a balance of attractive and repellent powers as to produce a triple compound; and by the extension of this reasoning, complicated chemical union may be easily explained.

Numerical illustrations of these notions might be made without difficulty, and they might be applied to all cases of chemical action; but in the present state of the inquiry, a great extension of this hypothetical part of the subject would be premature.

The general idea will, however, afford an easy explanation of the influence of affinity by the masses of the acting substances, as elucidated by the experiments of M. Berthollet; for the combined effect of many particles possessing a feeble electrical energy, may be conceived equal or even superior to the effect of a few particles possessing a strong electrical energy: and the facts mentioned, page 25, confirm the supposition:* for concentrated alkaline lixivia resist the transmission of acids by electricity much more powerfully than weak ones.

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* [Vide Col. Works, Elements of Chemical Philosophy, Vol. III. p. 84.]

Allowing combination to depend upon the balance of the natural electrical energies of bodies, it is easy to conceive that a *measure* may be found of the artificial energies, as to intensity and quantity produced in the common electrical machine, or the Voltaic apparatus, capable of destroying this equilibrium; and such a measure would enable us to make a scale of electrical powers corresponding to degrees of affinity.

In the circuit of the Voltaic apparatus, completed by metallic wires and water, the strength of the opposite electricities diminish from the points of contact of the wires towards the middle point in the water, which is necessarily neutral. In a body of water of considerable length it probably would not be difficult to assign the places in which the different neutral compounds yielded to, or resisted, decomposition. Sulphate of barytes, in all cases that I tried, required immediate contact with the wire: solution of sulphate of potash exhibited no marks of decomposition with the power of 150, when connected in a circuit of water ten inches in length, at four inches from the positive point; but when placed within two inches, its alkali was slowly repelled and its acid attracted.*

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* In this experiment, the water was contained in a circular glass bason two inches deep, the communication was made by pieces of amianthus of about the eighth of an inch in breadth. The saline solution filled a half ounce measure, and the distance between the solution and the water, at both points of communication, was a quarter of an inch. I mention these circumstances because the quantity of fluid and the extent of surface materially influence the result in trials of this kind. Water included in glass siphons forms a much less perfect conducting chain than when diffused upon the surface of fibrous nonconducting substances of much smaller volume than the diameter of the siphons. I attempted to employ siphons in some of my first experiments; but the very great inferiority of effect as compared with that of amianthus made me altogether relinquish the use of them.

Whenever bodies brought by artificial means into a high state of opposite electricities are made to restore the equilibrium, heat and light are the common consequences. It is perhaps an additional circumstance, in favour of the theory to state, that heat and light are likewise the result of all intense chemical action. And as in certain forms of the Voltaic battery, where large quantities of electricity of low intensity act, heat is produced without light; so in slow combinations there is an increase of temperature without luminous appearance.

The *effect of* HEAT, in producing combination, may be easily explained according to these ideas. It not only often gives more freedom of motion to the particles, but in a number of cases it seems to exalt the electrical energies of bodies; glass, the tourmalin, sulphur, all afford familiar instances of this last species of energy.

I heated together an insulated plate of copper and a plate of sulphur, and examined their electricities as their temperature became elevated: these electricities, scarcely sensible at 56° Fahrenheit to the condensing electrometer, became at 100° Fahrenheit capable of affecting the gold leaves without condensation: they increased in a still higher ratio as the sulphur approached towards its point of fusion. At a little above this point, as is well known from the experiments of the Dutch chemists, the two substances rapidly combine, and heat and light are evident.

Similar effects may be conceived to occur in the case of oxygen and hydrogen, which form water, a body apparently neutral in electrical energy to most other substances: and we may reasonably conclude that there is the same exaltation of power, in all cases of combustion.

In general, when the different energies are strong and in perfect equilibrium, the combination ought to be quick, the heat and light intense, and the new compound in a neutral state. This would seem to be the case in the instance just quoted; and in the circumstances of the union of the strong alkalies and acids. But where one energy is feeble and the other strong, all the effects must be less vivid; and the compound, instead of being neutral, ought to exhibit the excess of the stronger energy.

This last idea is confirmed by all the experiments which I have been able to make on the energies of the saline compounds with regard to the metals. Nitrate and sulphate of potash, muriate of lime, oxymuriate of potash, though repeatedly touched upon a large surface by plates of copper and zinc, gave no electrical charge to them; subcarbonate of soda and borax, on the contrary, gave a slight negative charge, and alum and superphosphate of lime a feeble positive charge.

Should this principle on further inquiry be found to apply generally, the degree of the electrical energies of bodies, ascertained by means of sensible instruments, will afford new and useful indications of their composition.

IX. *On the Mode of Action on the Pile of Volta, with Experimental Elucidations.*

The great tendency of the attraction of the different chemical agents, by the positive and negative surfaces in the Voltaic apparatus, seems to be to restore the electrical equilibrium. In a Voltaic battery, composed of copper, zinc, and solution of muriate of soda, all circulation of the electricity ceases, the equilibrium is restored if copper be brought in contact with the zinc on both sides: and oxygen and acids, which are attracted by the positively electrified zinc, exert similar agencies to the copper, but probably in a slighter degree, and being capable of combination with the metal, they produce a momentary equilibrium only.

The electrical energies of the metals with regard to each other, or the substances dissolved in the water, in the Voltaic and other analogous instruments, seem to be the causes that disturb the equilibrium, and the chemical changes the causes that tend to restore the equilibrium; and the phenomena most probably depend on their joint agency.

In the Voltaic pile of zinc, copper, and solution of muriate of soda, in what has been called its condition of electrical tension, the communicating plates of copper and zinc are in opposite electrical states. And with regard to electricities of such very low intensity, water is an insulating body: every copper plate consequently produces by induction an increase of positive electricity upon the opposite zinc plate; and every zinc plate an increase of negative electricity on the opposite copper plate: and the intensity increases with the number, and the quantity with the extent of the series.

When a communication is made between the two extreme points, the opposite electricities tend to annihilate each other; and if the fluid medium could be a substance incapable of decomposition, the equilibrium, there is every reason to believe, would be restored, and the motion of the electricity cease. But solution of muriate of soda being composed of two series of elements possessing opposite electrical energies, the oxygen and the acid are attracted by the zinc, and the hydrogen and the alkali by the copper. The balance of power is momentary only; for solution of zinc is formed, and the hydrogen disengaged. The negative energy of the copper and the positive energy of the zinc are consequently again exerted, enfeebled only by the opposing energy of the soda in contact with the copper, and the process of electromotion continues, as long as the chemical changes are capable of being carried on.

This theory in some measure reconciles the hypothetical principles of the action of the pile adopted by its illustrious inventor, with the opinions concerning the chemical origin of Galvanism, supported by the greater number of the British philosophers, and it is confirmed and strengthened by many facts and experiments.

Thus the Voltaic pile of 20 pairs of plates of copper and zinc exhibits no permanent electromotive power when the connecting fluid is water free from air;* for this substance does not readily undergo chemical change, and the equilibrium seems to be capable of being permanently restored through it. Concentrated sulphuric acid, which is a much more perfect conductor, is equally inefficient, for it has little action upon zinc, and is itself decomposed only by a very strong power. Piles, containing as their fluid element either pure water or sulphuric acid, will undoubtedly give single shocks, and this effect is connected with the restoration of the equilibrium disturbed by the energies of the metals; but when their extreme plates are connected there is no exhibition, as in usual cases of electromotion. Water containing loosely combined oxygen is more efficient than water containing common air, as it enables oxide of zinc to be formed more rapidly, and in larger quantities. Neutrosaline solutions which are at first very active, lose their energy in proportion as their acid arranges itself on the side of the zinc, and their alkali on that of the copper; and I have found the powers of a combination nearly destroyed from this cause very much revived, merely by agitating the fluids in the cells and mixing their parts together. Diluted acids, which are themselves easily decomposed, or which assist the decomposition of water, are above all other substances powerful; for they dissolve the zinc, and furnish only a gaseous product to the negative surface, which is immediately disengaged.

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* The experiments proving this fact, and the other analogous facts in this page, may be seen detailed [Col. Works, Vol. II. p. 157 & 167] in Nicholson's Journal, 4to. vol. iv. p. 338 & 394; and Phil. Mag. vol. x. p. 40.

There are other experiments connected with very striking results, which offer additional reasons for supposing the decomposition of the chemical menstrua essential to the continued electromotion in the pile.

As when an electrical discharge is produced by means of small metallic surfaces in the Voltaic battery, (the opposite state being exalted,) *sensible* heat is the consequence, it occurred to me, that if the decomposition of the chemical agents was essential to the balance of the opposed electricities, the effect, in a saline solution, of this decomposition, and of the transfer of the alkali to the negative side, and of the acid to the positive side, ought, under favourable circumstances, to be connected with an increase of temperature.

I placed the gold cones, which have been so often mentioned, in the circuit of the battery with the power of 100, I filled them with distilled water, and connected them by a piece of moistened asbestus, about an inch in length and $\frac{1}{8}$ of an inch diameter; I provided a small air-thermometer capable of being immersed in the gold cones, expecting (if any) only a very slight change of temperature; I introduced a drop of solution of sulphate of potash into the positive cone: the decomposition instantly began: potash passed rapidly over into the negative cone, heat was immediately sensible; and in less than two minutes the water was in a state of ebullition.

I tried the same thing with a solution of nitrate of ammonia, and in this instance the heat rose to such an intensity as to evaporate all the water in three or four minutes, with a kind of explosive noise; and at last actual inflammation took place, with the decomposition and dissipation of the greatest part of the salt.*

That the increase of the conducting power of the water by the drop of saline solution had little or nothing to do with the effect, is evident from this circumstance. I introduced a quantity of strong lixivium of potash into the cones, and likewise concentrated sulphuric acid, separately, which are better conductors than solutions of the neutral salts; but there was very little sensible effect.

The same principles will apply to all the varieties of the electrical apparatus, whether containing double or single plates; and if the ideas developed in the preceding sections be correct, one property operating under different modifications is the universal cause of their activity.

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* In this process ammonia was rapidly given off from the surface of the negative cone, and nitrous acid from that of the positive cone, and a white vapour was produced by their combination in the atmosphere above the apparatus.

X. *On some general Illustrations and Applications of the foregoing Facts and Principles; and Conclusion.*

The general ideas advanced in the preceding pages are evidently directly in contradiction to the opinion advanced by Fabroni, and which, in the early stage of the investigation, appeared extremely probable, namely, that chemical changes are the *primary* causes of the phenomena of Galvanism.

Before the experiments of M. Volta on the electricity excited by the mere contact of metals were published, I had to a certain extent adopted this opinion; but the new facts immediately proved that another power must necessarily be concerned; for it was not possible to refer the electricity exhibited by the apposition of metallic surfaces to any chemical alterations, particularly as the effect is more distant in a dry atmosphere, in which even the most oxidable metals do not change, than in a moist one, in which many metals undergo chemical alteration.

Other facts likewise soon occurred demonstrative of the same thing. In the Voltaic combination of diluted nitrous acid, zinc and copper, as is well known, the side of the zinc exposed to the acid is positive. But in combinations of zinc, water and diluted nitric acid, the surface exposed to the acid is negative; though if the *chemical* action of the acid on the zinc had been the cause of the effect, it ought to be the same in both cases.

In mere cases of chemical change, likewise, electricity is never exhibited. Iron burnt in oxygen gas, properly connected with a condensing electrometer, gives no charge to it during the process. Nitre and charcoal deflagrated in communication with the same instrument do not by their agencies in the slightest degree affect the gold leaves. Solid pure potash and sulphuric acid made to combine in an insulated platina crucible produce no electrical appearances. A solid amalgam of bismuth and a solid amalgam of lead become fluid when mixed together: the experiment, I find, is connected with a diminution of temperature, but with no exhibition of electrical effects. A thin plate of zinc, after being placed upon a surface of mercury, and separated by an insulated body, is found positive, the mercury is negative: the effects are exalted by heating the metals: but let them be kept in contact sufficiently long to amalgamate, and the compound gives no signs of electricity. I could mention a great number of other instances of *pure chemical* action in which I have used all the means in my power to ascertain the fact, and the result has been constantly the same. In cases of effervescence, indeed, particularly when accompanied by much heat, the metallic vessels employed become negative, but this is a phenomenon connected with *evaporation*, the change of state of a body independent of chemical change, and is to be referred to a different law.*

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* The change of the capacities of bodies in consequence of the alteration in their volumes, or states of existence by heat, is a continually operating source of electrical effects: and as I have hinted, page 36, it often interferes with the results of experiments on the electrical energies of bodies as exhibited by contact. It is likewise probably one of the sources of the capricious results of experiments of friction, in which the same body, according as its texture is altered, or its temperature changed, assumes different states with regard to another body. Friction may be considered as a succession of contacts, and the natural energies of bodies would probably be accurately exhibited by it, if the unequal excitation of heat or its unequal communication to the different surfaces did not interfere by altering unequally their electrical capacities. Of the

I mentioned the glass plates of Beccaria as affording a parallel to the case of combination in consequence of the different electrical states of bodies. In Guyton de Morveau's experiments on cohesion, the different metals are said to have adhered to mercury with a force proportional to their chemical affinities. But the other metals have different electrical energies, or different degrees of the same electrical energy with regard to this body; and in all cases of contact of mercury with another metal, upon a large surface, they ought to adhere in consequence of the difference of their electrical states, and that with a force proportional to the exaltation of those states. Iron, which M. Guyton found slightly adhesive, I find exhibits little positive electricity after being laid upon a surface of mercury, and then separated. Tin, zinc, and copper, which adhere much more strongly, communicate higher charges to the condensing electrometer: I have had no instrument sufficiently exact to measure the differences: but it would seem that the adhesion from the difference of electrical states must have operated in these experiments,* which being proportional to the electrical energies are, on the hypothesis before stated, proportional to the chemical affinities. How far cohesion in general may be influenced or occasioned by this effect of the difference of the electrical energies of bodies is a curious question for investigation.

Many applications of the general facts and principles to the processes of chemistry, both in art and in nature,

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elements of flint glass, silex is slightly negative with regard to the metals, the soda is positive; and in contacts of glass with metals I find it exhibits the excess of the energy of the alkali: the case, as is well known, is the same in friction, the amalgam of the common machine is essential to its powerful excitation.

* Amalgamation undoubtedly must have interfered; but the *general* result seems to have been distinct.

will readily suggest themselves to the philosophical inquirer.

They offer very easy methods of separating acid and alkaline matter, when they exist in combination, either together or separately, in minerals; and the electrical powers of decomposition may be easily employed in animal and vegetable analysis.

A piece of muscular fibre, of two inches long and half an inch in diameter, after being electrified by the power of 150 for five days, became perfectly dry and hard, and left on incineration no saline matter. Potash, soda, ammonia, lime, and oxide of iron were evolved from it on the negative side, and the three common mineral acids and the phosphoric acid, were given out on the positive side.

A laurel leaf treated in the same manner, appeared as if it had been exposed to a heat of 500° or 600° Fahrenheit, and was brown and parched. Green colouring matter, with resin, alkali, and lime, appeared in the negative vessel: and the positive vessel contained a clear fluid, which had the smell of peach blossoms; and which, when neutralized by potash, gave a blue-green precipitate to solution of sulphate of iron; so that it contained vegetable prussic acid.

A small plant of mint, in a state of healthy vegetation, was made the medium of connection in the battery, its extremities being in contact with pure water; the process was carried on for 10 minutes: potash and lime were found in the negatively electrified water, and acid matter in the positively electrified water, which occasioned a precipitate in solutions of muriate of barytes, nitrate of silver, and muriate of lime. This plant recovered after the process: but a similar one, that had been electrified for four hours with like results, faded and died.* The facts shew that the electrical powers of decomposition act even upon living vegetable matter: and there are some phenomena which seem to prove that they operate likewise upon living animal systems. When the fingers, after having been carefully washed with pure water, are brought in contact with this fluid in the positive part of the circuit, acid matter is rapidly developed, having the characters of a mixture of muriatic, phosphoric, and sulphuric acids: and if a similar trial be made in the negative part, fixed alkaline matter is as quickly exhibited.

The acid and alkaline tastes produced upon the tongue, in Galvanic experiments, seem to depend upon the decomposition of the saline matter contained in the living animal substance, and perhaps in the saliva.

As acid and alkaline substances are capable of being separated from their combinations in living systems by electrical powers, there is every reason to believe that by converse methods they may be likewise introduced into the animal economy, or made to pass through the animal organs: and the same thing may be supposed of metallic oxides; and these ideas ought to lead to some new investigations in medicine and physiology.

It is not improbable that the electrical decomposition of the neutral salts in different cases may admit of economical uses. Well burned charcoal and plumbago, or charcoal and iron, might be made the exciting powers; and such an arrangement, if erected upon an extensive scale, neutrosaline matter being employed in every series, would, there is every reason to believe, produce large quantities of acids and alkalies with very little trouble or expense.

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* Seeds, I find, when placed in pure water in the positive part of the circuit, germinate much more rapidly than under common circumstances; but in the negative part of the circuit they do not germinate at all. Without supposing any peculiar effects from the different electricities, which however *may* operate, the phenomenon may be accounted for from the saturation of the water near the positive metallic surface with oxygen, and of that near the negative surface with hydrogen.

Ammonia and acids capable of decomposition, undergo chemical change in the Voltaic circuit only when they are in very concentrated solution, and in other cases are merely carried to their particular points of rest. This fact may induce us to hope that the new mode of analysis may lead us to the discovery of the *true* elements of bodies, if the materials acted on be employed in a certain state of concentration, and the electricity be sufficiently exalted. For if chemical union be of the nature which I have ventured to suppose, however strong the natural electrical energies of the elements of bodies may be, yet there is every probability of a limit to their strength: whereas the powers of our artificial instruments seem capable of indefinite increase.

Alterations of electrical equilibrium are continually taking place in nature; and it is probable that this influence, in its faculties of decomposition and transference, considerably interferes with the chemical alterations occurring in different parts of our system.

The electrical appearances which precede earthquakes and volcanic eruptions, and which have been described by the greater number of observers of these awful events, admit of very easy explanation on the principles that have been stated.

Besides the cases of sudden and violent change, there must be constant and tranquil alterations in which electricity is concerned, produced in various parts of the interior strata of our globe.

Where pyritous strata and strata of coal-blende occur, where the pure metals or the sulphurets are found in contact with each other, or any conducting substances, and where different strata contain different saline menstrua, electricity must be continually manifested;* and it is very probable, that many mineral formations have been materially influenced, or even occasioned by its agencies.

In an experiment that I made of electrifying a mixed solution of muriates of iron, of copper, of tin, and of cobalt, in a positive vessel, distilled water being in a negative vessel, all the four oxides passed along the asbestus, and into the negative tube, and a yellow metallic crust formed on the wire, and the oxides arranged themselves in a mixed state round the base of it.

In another experiment, in which carbonate of copper was diffused through water in a state of minute division, and a negative wire placed in a small perforated cube of zeolite in the water, green crystals collected round the cube; the particles not being capable of penetrating it.

By a multiplication of such instances the electrical power of transference may be easily conceived to apply to the explanation of some of the principal and most mysterious facts in geology.†

And by imagining a scale of feeble powers, it would be easy to account for the association of the insoluble metallic and earthy compounds, containing acids.

Natural electricity has hitherto been little investigated, except in the case of its evident and powerful concentration in the atmosphere.

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* [This conclusion has been amply confirmed by later researches; especially those of Mr. Fox on the electrical condition of metallic veins in the mines of Cornwall.]

† [Such instances have been multiplied of late years to a considerable extent; especially by the ingenious researches of M. Becquerel on electrochemical agency of low intensity, all in conformity with the above remark.]

Its slow and silent operations in every part of the surface will probably be found more immediately and importantly connected with the order and economy of nature; and investigations on this subject can hardly fail to enlighten our philosophical systems of the earth; and may possibly place new powers within our reach.*

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* [The minor prize of 3,000 francs, founded by Napoleon when first Consul, for the most important results in electrical research during each year, was awarded by the Institute, to the author, for this paper: the principal prize of 60,000 francs, of which the preceding was only the interest, in the opinion of the best judges, was rather due to him, as it was proposed to be given "à celui qui, par ses expériences et ses découvertes, fera à faire à l'Electricité et au Galvanisme un pas comparable à cela qu'ont fait faire à ces sciences Franklin et Volta." Thus the writer in the Quarterly Review, already referred to, remarks: "it was only questioned by those who were capable of appreciating its importance (that of this Bakerian Lecture) whether they acted with strict impartiality in assigning to him the annual interest only, while he appeared to have a fair claim to the principal."]

EXPLANATION OF THE FIGURES.

PLATE I.

Fig. 1, represents the agate cup, mentioned page 4.

Fig. 2, represents the gold cones, page 6.

Fig. 3, represents the glass tubes, and their attached apparatus, page 20.

Fig. 4, represents the two glass tubes, with the intermediate vessel, page 21.

In all the figures AB denote the wires, rendered one positively, the other negatively electrical; and C the connecting pieces of moistened amianthus.

PLATE I

Fig. 1

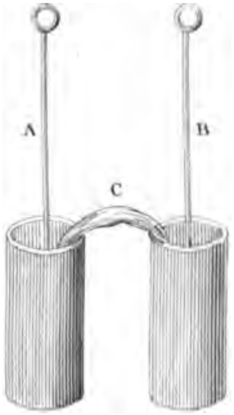


Fig. 2.

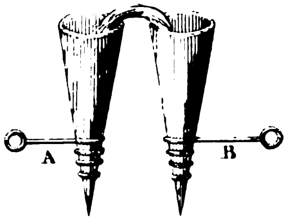


Fig. 3.

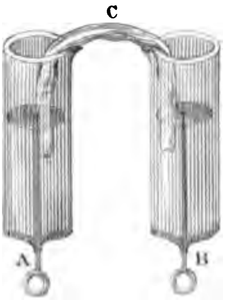
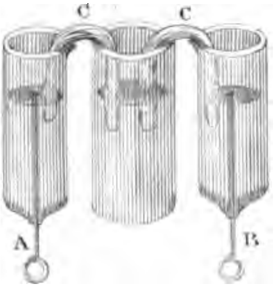


Fig. 4



II.

ON SOME NEW PHENOMENA OF CHEMICAL CHANGES PRODUCED BY ELECTRICITY, PARTICULARLY THE DECOMPOSITION OF THE FIXED ALKALIES, AND THE EXHIBITION OF THE NEW SUBSTANCES WHICH CONSTITUTE THEIR BASES; AND ON THE GENERAL NATURE OF ALKALINE BODIES.*

I. *Introduction.*

In the Bakerian Lecture which I had the honour of presenting to the Royal Society last year, I described a number of decompositions and chemical changes produced in substances of known composition by electricity, and I ventured to conclude from the general principles on which the phenomena were capable of being explained, that the new methods of investigation promised to lead to a more intimate knowledge than had hitherto been obtained, concerning the true elements of bodies.

This conjecture, then sanctioned only by strong analogies, I am now happy to be able to support by some conclusive facts. In the course of a laborious experimental application of the powers of electro-chemical analysis, to bodies which have appeared simple when examined by common chemical agents, or which at least have never been decomposed, it has been my good fortune to obtain new and singular results.

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* [From Phil. Trans. for 1808; read before the Royal Society, Nov. 19, 1807.]

Such of the series of experiments as are in a tolerably mature state, and capable of being arranged in a connected order, I shall detail in the following sections, particularly those which demonstrate the decomposition and composition of the fixed alkalies, and the production of the new and extraordinary bodies which constitute their bases.

In speaking of novel methods of investigation, I shall not fear to be minute. When the common means of chemical research have been employed, I shall mention only results. A historical detail of the progress of the investigation, of all the difficulties that occurred, and of the manner in which they were overcome, and of all the manipulations employed, would far exceed the limits assigned to this Lecture. It is proper to state, however, that when general facts are mentioned, they are such only as have been deduced from processes carefully performed and often repeated.

II. *On the Methods used for the Decomposition of the fixed Alkalies.*

The researches I had made on the decomposition of acids, and of alkaline and earthy neutral compounds, proved that the powers of electrical decomposition were proportional to the strength of the opposite electricities in the circuit, and to the conducting power and degree of concentration of the materials employed.

In the first attempts that I made on the decomposition of the fixed alkalies, I acted upon aqueous solutions of potash and soda, saturated at common temperatures, by the highest electrical power I could command,

and which was produced by a combination of Voltaic batteries belonging to the Royal Institution, containing 24 plates of copper and zinc of 12 inches square, 100 plates of 6 inches, and 150 of 4 inches square, charged with solutions of alum and nitrous acid; but in these cases, though there was a high intensity of action, the water of the solutions alone was affected, and hydrogen and oxygen disengaged with the production of much heat and violent effervescence.

The presence of water appearing thus to prevent any decomposition, I used potash in igneous fusion. By means of a stream of oxygen gas from a gasometer applied to the flame of a spirit lamp, which was thrown on a platina spoon containing potash, this alkali was kept for some minutes in a strong red heat, and in a state of perfect fluidity. The spoon was preserved in communication with the positive side of the battery of the power of 100 of 6 inches, highly charged; and the connection from the negative side was made by a platina wire.

By this arrangement some brilliant phenomena were produced. The potash appeared a conductor in a high degree, and as long as the communication was preserved, a most intense light was exhibited at the negative wire, and a column of flame, which seemed to be owing to the developement of combustible matter, arose from the point of contact.

When the order was changed, so that the platina spoon was made negative, a vivid and constant light appeared at the opposite point: there was no effect of inflammation round it; but aëriform globules, which inflamed in the atmosphere, rose through the potash.

The platina, as might have been expected, was considerably acted upon; and in the cases when it had been negative, in the highest degree.

The alkali was apparently dry in this experiment; and it seemed probable that the inflammable matter arose from its decomposition. The residual potash was unaltered; it contained indeed a number of dark grey metallic particles, but these proved to be derived from the platina.

I tried several experiments on the electrization of potash rendered fluid by heat, with the hopes of being able to collect the combustible matter, but without success; and I only attained my object, by employing electricity as the common agent for fusion and decomposition.

Though potash, perfectly dried by ignition, is a non-conductor, yet it is rendered a conductor, by a very slight addition of moisture, which does not perceptibly destroy its aggregation; and in this state it readily fuses and decomposes by strong electrical powers.

A small piece of pure potash, which had been exposed for a few seconds to the atmosphere, so as to give conducting power to the surface, was placed upon an insulated disc of platina, connected with the negative side of the battery of the power of 250 of 6 and 4, in a state of intense activity; and a platina wire, communicating with the positive side, was brought in contact with the upper surface of the alkali. The whole apparatus was in the open atmosphere.

Under these circumstances a vivid action was soon observed to take place. The potash began to fuse at both its points of electrization. There was a violent effervescence at the upper surface; at the lower, or negative surface, there was no liberation of elastic fluid; but small globules having a high metallic lustre, and being precisely similar in visible characters to quicksilver, appeared, some of which burnt with explosion and bright flame, as soon as they were formed, and others remained, and were merely tarnished, and finally covered by a white film which formed on their surfaces.*

These globules, numerous experiments soon shewed to be the substance I was in search of, and a peculiar inflammable principle the basis of potash. I found that the platina was in no way connected with the result, except as the medium for exhibiting the electrical powers of decomposition; and a substance of the same kind was produced when pieces of copper, silver, gold, plumbago, or even charcoal were employed for completing the circuit.

The phenomenon was independent of the presence of air; I found that it took place when the alkali was in the vacuum of an exhausted receiver.

The substance was likewise produced from potash fused by means of a lamp, in glass tubes confined by mercury, and furnished with hermetically inserted platina wires by which the electrical action was transmitted. But this operation could not be carried on for any considerable time; the glass was rapidly dissolved by the action of the alkali, and this substance soon penetrated through the body of the tube.

Soda, when acted upon in the same manner as potash, exhibited an analogous result: but the decomposition demanded greater intensity of action in the batteries, or the alkali was required to be in much thinner and smaller pieces. With the battery of 100 of 6 inches in full activity I obtained good results from pieces of potash weighing from 40 to 70 grains, and of a thickness which made the distance of the electrified metallic surfaces nearly a quarter of an inch; but with a similar power it was impossible to produce the effects of decomposition on pieces of soda of more than 15 or 20 grains in weight, and that only when the distance between the wires was about $\frac{1}{8}$ or $\frac{1}{10}$ of an inch.

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* [In a Manuscript Lecture it is stated by the author, that he discovered Potassium on the 6th October 1807, and Sodium a few days after. Vide Vol. I. p. 109.]

The substance produced from potash remained fluid at the temperature of the atmosphere at the time of its production; that from soda, which was fluid in the degree of heat of the alkali during its formation, became solid on cooling, and appeared having the lustre of silver.

When the power of 250 was used, with a very high charge for the decomposition of soda, the globules often burnt at the moment of their formation, and sometimes violently exploded and separated into smaller globules, which flew with great velocity through the air in a state of vivid combustion, producing a beautiful effect of continued jets of fire.*

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* [Recently some able writers on the subject of the progress of science, in noticing the decomposition of the fixed alkalies by Voltaic electricity, have attributed the success of the author to the supposed very powerful batteries of the Laboratory of the Royal Institution—confounding, as I apprehend they have done, the really very powerful apparatus, [that of 2000 double plates described in the Elements of Chemical Philosophy, page 110,] constructed subsequently to the discoveries alluded to, with the means at his disposal, as stated in the text; and which, from his manner of using them—bringing together to produce a joint effect, instruments of different sizes, indicates clearly, that he could not depend on the power of any one battery to effect that separation of elements on which he calculated, according to the principles of electro-chemical decomposition, established by his former researches. Neither at the time, nor till recently, was the merit of the discovery considered as diminished by the favourable circumstances under which it was made, and which he himself, in the spirit of the Baconian philosophy, amply ac-

III. *Theory of the Decomposition of the fixed Alkalies; their Composition, and Production.*

As in all decompositions of compound substances which I had previously examined, at the same time that combustible bases were developed at the negative surface in the electrical circuit, oxygen was produced, and evolved or carried into combination at the positive surface, it was reasonable to conclude that this substance was generated in a similar manner by the electrical action upon the alkalies; and a number of experiments made above mercury, with the apparatus for excluding external air, proved that this was the case.

When solid potash, or soda in its conducting state,

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knowledged, when remarking, in relation to the discoveries effected by electro-chemical decomposition, in the introduction to his Elements of Chemical Philosophy, "that the native intellectual powers of men in different times, are not so much the causes of the different success of their labours, as the peculiar nature of the means and artificial resources in their possession." An acknowledgment which the able critic in the Quarterly Review for 1812, before alluded to, notices as an example of laudable modesty; observing as justly as liberally, "It must be remembered, that almost every other discovery of importance, which has been made in science, has been facilitated by some previous steps, which have rendered practicable what might otherwise have presented insuperable obstacles to human ingenuity; nor has such a preparation ever been allowed to detract from the just applause bestowed on those who have been distinguished from their contemporaries by a more successful exertion of talent." Unless men of science are actuated by this feeling of justice, in the bestowing of honour, when deserved, in full and free measure, they should not find fault with the neglect of science on the part of government. Flattery is not applicable to the dead; but admiration of their deeds may stimulate the living. He is not to be envied, who would detract from the merit of the elder Herschell, because he did not invent the telescope, or from that of the majority of the distinguished men of this, and of former periods, who have employed in their researches, means previously existing.]

was included in glass tubes furnished with electrified platina wires, the new substances were generated at the negative surfaces; the gas given out at the other surface proved by the most delicate examination to be pure oxygen; and unless an excess of water was present, no gas was evolved from the negative surface.

In the synthetical experiments, a perfect coincidence likewise will be found.

I mentioned that the metallic lustre of the substance from potash immediately became destroyed in the atmosphere, and that a white crust formed upon it. This crust I soon found to be pure potash, which immediately deliquesced, and new quantities were formed, which in their turn attracted moisture from the atmosphere till the whole globule disappeared, and assumed the form of a saturated solution of potash.*

When globules were placed in appropriate tubes containing common air or oxygen gas confined by mercury, an absorption of oxygen took place; a crust of alkali instantly formed upon the globule; but from the want of moisture for its solution, the process stopped, the interior being defended from the action of the gas.

With the substance from soda, the appearances and effects were analogous.

When the substances were strongly heated, confined in given portions of oxygen, a rapid combustion with a brilliant white flame was produced, and the metallic globules were found converted into a white and solid mass, which in the case of the substance from potash was found to be potash, and in the case of that from soda, soda.

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* Water likewise is decomposed in the process. We shall hereafter see that the bases of the fixed alkalies act upon this substance with greater energy than any other known bodies. The minute theory of the oxidation of the bases of the alkalies in the free air, is this:—oxygen gas is first attracted by them, and alkali formed. This alkali speedily absorbs water. This water is again decomposed. Hence, during the conversion of a globule into alkaline solution, there is a constant and rapid disengagement of small quantities of gas.

Oxygen gas was absorbed in this operation, and nothing emitted which affected the purity of the residual air.

The alkalies produced were apparently dry, or at least contained no more moisture than might well be conceived to exist in the oxygen gas absorbed; and their weights considerably exceeded those of the combustible matters consumed.

The processes on which these conclusions are founded will be fully described hereafter, when the minute details which are necessary will be explained, and the proportions of oxygen, and of the respective inflammable substances which enter into union to form the fixed alkalies, will be given.

It appears then, that in these facts there is the same evidence for the decomposition of potash and soda into oxygen and two peculiar substances, as there is for the decomposition of sulphuric and phosphoric acids and the metallic oxides into oxygen and their respective combustible bases.

In the analytical experiments, no substances capable of decomposition are present but the alkalies and a minute portion of moisture; which seems in no other way essential to the result, than in rendering them conductors at the surface: for the new substances are not generated till the interior, which is dry, begins to be fused; they explode when in rising through the fused alkali they come in contact with the heated moistened surface; they cannot be produced from crystallized alkalies, which contain much water; and the effect produced by the electrization of ignited potash, which contains no sensible quantity of water, confirms the opinion of their formation independently of the presence of this substance.

The combustible bases of the fixed alkalies seem to be repelled as other combustible substances, by positively electrified surfaces, and attracted by negatively electrified surfaces, and the oxygen follows the contrary order;* or the oxygen being naturally possessed of the negative energy, and the bases of the positive, do not remain in combination when either of them is brought into an electrical state opposite to its natural one. In the synthesis, on the contrary, the natural energies or attractions come in equilibrium with each other; and when these are in a low state at common temperatures, a slow combination is effected; but when they are exalted by heat, a rapid union is the result; and, as in other like cases, with the production of fire.—A number of circumstances relating to the agencies of the bases of the alkalies will be immediately stated, and will be found to offer confirmations of these general conclusions.

IV. *On the Properties and Nature of the Basis of Potash.*

After I had detected the bases of the fixed alkalies, I had considerable difficulty to preserve and confine them so as to examine their properties, and submit them to experiments; for, like the *alkahests* imagined by the alchemists, they acted more or less upon almost every body to which they were exposed.

The fluid substance, amongst all those I have tried, on which I find they have least effect, is recently distilled naphtha.—In this material, when excluded from the air, they remain for many days without considerably changing, and their physical properties may be easily examined in the atmosphere when they are covered by a thin film of it.

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* See Bakerian Lecture 1806, p. 28.

The basis of potash at 60° Fahrenheit, the temperature in which I first examined it, appeared, as I have already mentioned, in small globules possessing the metallic lustre, opacity, and general appearance of mercury; so that when a globule of mercury was placed near a globule of the peculiar substance, it was not possible to detect a difference by the eye.

At 60° Fahrenheit it is however only imperfectly fluid, for it does not readily run into a globule when its shape is altered; at 70° it becomes more fluid; and at 100° its fluidity is perfect, so that different globules may be easily made to run into one. At 50° Fahrenheit it becomes a soft and malleable solid, which has the lustre of polished silver; and at about the freezing point of water it becomes harder and brittle, and when broken in fragments, exhibits a crystallized texture, which in the microscope seems composed of beautiful facets of a perfect whiteness and high metallic splendour.

To be converted into vapour, it requires a temperature approaching that of the red heat; and when the experiment is conducted under proper circumstances, it is found unaltered after distillation.

It is a perfect conductor of electricity. When a spark from the Voltaic battery of 100 of 6 inches is taken upon a large globule in the atmosphere, the light is green, and combustion takes place at the point of contact only. When a small globule is used, it is completely dissipated with explosion, accompanied by a most vivid flame, into alkaline fumes.

It is an excellent conductor of heat.

Resembling the metals in all these sensible properties, it is however remarkably different from any of them in specific gravity; I found that it rose to the surface of naphtha distilled from petroleum, and of which the specific gravity was ·861, and it did not sink in double distilled naphtha, the specific gravity of which was about ·770, that of water being considered as 1. The small quantities in which it is produced by the highest electrical powers, rendered it very difficult to determine this quality with minute precision. I endeavoured to gain approximations on the subject by comparing the weights of perfectly equal globules of the basis of potash and mercury. I used the very delicate balance of the Royal Institution, which when loaded with the quantities I employed, and of which the mercury never exceeded ten grains, is sensible at least to the $\frac{1}{1000}$ of a grain. Taking the mean of 4 experiments, conducted with great care, its specific gravity at 62° Fahrenheit, is to that of mercury as 10 to 223, which gives a proportion to that of water nearly as 6 to 10; so that it is the lightest fluid body known. In its solid form it is a little heavier, but even in this state when cooled to 40° Fahrenheit, it swims in the double-distilled naphtha.

The chemical relations of the basis of potash are still more extraordinary than its physical ones.

I have already mentioned its alkalization and combustion in oxygen gas.—It combines with oxygen slowly and without flame at all temperatures that I have tried below that of its vaporization.—But at this temperature combustion takes place, and the light is of a brilliant whiteness and the heat intense. When heated slowly in a quantity of oxygen gas not sufficient for its complete conversion into potash, and at a temperature inadequate to its inflammation, 400° Fahrenheit, for instance, its tint changes to that of a red brown, and when the heat is withdrawn, all the oxygen is found to be absorbed, and a solid is formed of a greyish colour, which partly consists of potash and partly of the basis of potash in a lower degree of oxigenation,*—and which becomes potash by being exposed to water, or by being again heated in fresh quantities of air.

The substance consisting of the basis of potash combined with an under proportion of oxygen, may likewise be formed by fusing dry potash and its basis together under proper circumstances.—The basis rapidly loses its metallic splendour; the two substances unite into a compound, of a red brown colour when fluid, and of a dark grey hue when solid; and this compound soon absorbs its full proportion of oxygen when exposed to the air, and is wholly converted into potash.

And the same body is often formed in the analytical experiments when the action of the electricity is intense, and the potash much heated.

The basis of potash, when introduced into oximuriatic acid gas, burns spontaneously with a bright red light, and a white salt proving to be muriate of potash is formed.

When a globule is heated in hydrogen at a degree below its point of vaporization, it seems to dissolve in it, for the globule diminishes in volume, and the gas explodes with alkaline fumes and bright light, when suffered to pass into the air; but by cooling, this spontaneous detonating property is destroyed, and the basis is either wholly or principally deposited.

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* [This inference was not confirmed by after and more careful inquiry: in the experiments above mentioned the red tint was probably owing to the production of a thin crust of peroxide: vide the author's paper "On some of the Combinations of oximuriatic gas," &c. written in 1810.]

The action of the basis of potash on water exposed to the atmosphere is connected with some beautiful phenomena. When it is thrown upon water, or when it is brought into contact with a drop of water at common temperatures, it decomposes it with great violence, an instantaneous explosion is produced with brilliant flame, and a solution of pure potash is the result.

In experiments of this kind, an appearance often occurs similar to that produced by the combustion of phosphuretted hydrogen; a white ring of smoke, which gradually extends as it rises into the air.

When water is made to act upon the basis of potash out of the contact of air and preserved by means of a glass tube under naphtha, the decomposition is violent; and there is much heat and noise, but no luminous appearance, and the gas evolved when examined in the mercurial or water pneumatic apparatus is found to be pure hydrogen.

When a globule of the basis of potash is placed upon ice, it instantly burns with a bright flame, and a deep hole is made in the ice, which is found to contain a solution of potash.

The theory of the action of the basis of potash upon water exposed to the atmosphere, though complicated changes occur, is far from being obscure. The phenomena seem to depend on the strong attractions of the basis for oxygen and of the potash formed for water. The heat, which arises from two causes, decomposition and combination, is sufficiently intense to produce the inflammation. Water is a bad conductor of heat; the globule swims exposed to air; a part of it, there is the greatest reason to believe, is dissolved by the heated nascent hydrogen; and this substance being capable of spontaneous inflammation, explodes, and communicates the effect of combustion to any of the basis that may be yet uncombined.

When a globule confined out of the contact of air is acted upon by water, the theory of decomposition is very simple, the heat produced is rapidly carried off, so that there is no ignition; and a high temperature being requisite for the solution of the basis in hydrogen, this combination probably does not take place, or at least it can have a momentary existence only.

The production of alkali in the decomposition of water by the basis of potash is demonstrated in a very simple and satisfactory manner by dropping a globule of it upon moistened paper tinged with turmeric. At the moment that the globule comes into contact with the water, it burns, and moves rapidly upon the paper, as if in search of moisture, leaving behind it a deep reddish brown trace, and acting upon the paper precisely as dry caustic potash.

So strong is the attraction of the basis of potash for oxygen, and so great the energy of its action upon water, that it discovers and decomposes the small quantities of water contained in alcohol and ether, even when they are carefully purified.

In ether this decomposition is connected with an instructive result. Potash is insoluble in this fluid; and when the basis of potash is thrown into it, oxygen is furnished to it, and hydrogen gas disengaged, and the alkali as it forms renders the ether white and turbid.

In both these inflammable compounds the energy of its action is proportional to the quantity of water they contain, and hydrogen and potash are the constant result.

The basis of potash, when thrown into solutions of the mineral acids, inflames and burns on the surface. When it is plunged by proper means beneath the surface envèloped in potash, surrounded by naphtha, it acts upon the oxygen with the greatest intensity, and all its effects are such as may be explained from its strong affinity for this substance. In sulphuric acid a white saline substance with a yellow coating, which is probably sulphate of potash surrounded by sulphur, and a gas which has the smell of sulphurous acid, and which probably is a mixture of that substance with hydrogen gas, are formed. In nitrous acid, nitrous gas is disengaged, and nitrate of potash formed.

The basis of potash readily combines with the simple inflammable solids, and with the metals; with phosphorus and sulphur, it forms compounds similar to the metallic phosphurets and sulphurets.

When it is brought in contact with a piece of phosphorus, and pressed upon, there is a considerable action: they become fluid together, burn, and produce phosphate of potash. When the experiment is made under naphtha, their combination takes place without the liberation of any elastic matter, and they form a compound which has a considerably higher point of fusion than its two constituents, and which remains a soft solid in boiling naphtha. In its appearance it perfectly agrees with a metallic phosphuret, it is of the colour of lead, and when spread out, has a lustre similar to polished lead. When exposed to air of common temperatures, it slowly combines with oxygen, and becomes phosphate of potash. When heated upon a plate of platina, fumes exhale from it, and it does not burn till it attains the temperature of the rapid combustion of the basis of potash.

When the basis of potash is brought in contact with sulphur in fusion, in tubes filled with the vapour of naphtha, they combine rapidly with the evolution of heat and light, and a grey substance, in appearance like artificial sulphuret of iron, is formed, which if kept in fusion, rapidly dissolves the glass, and becomes bright brown. When this experiment is made in a glass tube hermetically sealed, no gas is liberated if the tube is opened under mercury; but when it is made in a tube connected with a mercurial apparatus, a small quantity of sulphuretted hydrogen is evolved, so that the phenomena are similar to those produced by the union of sulphur with the metals in which sulphuretted hydrogen is likewise disengaged, except that the ignition is stronger.* When the union is effected in the atmosphere, a great inflammation takes place, and sulphuret of potash is formed. The sulphuretted basis likewise gradually becomes oxigenated by exposure to the air, and is finally converted into sulphate.

The new substance produces some extraordinary and beautiful results with mercury. When one part of it is added to eight or ten parts of mercury in volume at 60°

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* The existence of hydrogen in sulphur, is rendered very probable by the ingenious researches of M. Berthollet, Jun. Annales de Chimie, Fevrier 1807, page 143. The fact is almost demonstrated by an experiment which I saw made by W. Clayfield, Esq. at Bristol, in 1799. Copper filings and powdered sulphur, in weight in the proportion of three to one rendered very dry, were heated together in a retort, connected with a mercurial pneumatic apparatus. At the moment of combination a quantity of elastic fluid was liberated amounting to nine or ten times the volume of the materials employed, and which consisted of sulphuretted hydrogen mixed with sulphurous acid. The first mentioned product, there is every reason to believe, must be referred to the sulphur, the last probably to the copper, which it is easy to conceive may have become slightly and superficially oxidated during the processes of filing and drying by heat.

Fahrenheit, they instantly unite and form a substance exactly like mercury in colour, but which seems to have less coherence, for small portions of it appear as flattened spheres. When a globule is made to touch a globule of mercury about twice as large, they combine with considerable heat; the compound is fluid at the temperature of its formation; but when cool it appears as a solid metal, similar in colour to silver. If the quantity of the basis of potash is still farther increased, so as to be about $\frac{1}{30}$th the weight of the mercury, the amalgam increases in hardness, and becomes brittle. The solid amalgam, in which the basis is in the smallest proportion, seems to consist of about one part in weight of basis and seventy parts of mercury, and is very soft and malleable.

When these compounds are exposed to air, they rapidly absorb oxygen; potash which deliquesces is formed; and in a few minutes the mercury is found pure and unaltered.

When a globule of the amalgam is thrown into water, it rapidly decomposes it with a hissing noise; potash is formed, pure hydrogen disengaged, and the mercury remains free.

The fluid amalgam of mercury and this substance dissolves all the metals I have exposed to it; and in this state of union, mercury acts on iron and platina.

When the basis of potash is heated with gold, or silver, or copper, in a close vessel of pure glass, it rapidly acts upon them; and when the compounds are thrown into water, this fluid is decomposed, potash formed, and the metals appear to be separated unaltered.

The basis of potash combines with fusible metal, and forms an alloy with it, which has a higher point of fusion than the fusible metal.

The action of the basis of potash upon the inflammable oily compound bodies, confirms the other facts of the strength of its attraction for oxygen.

On naphtha colourless and recently distilled, as I have already said, it has very little power of action; but in naphtha that has been exposed to the air it soon oxidates, and alkali is formed, which unites with the naphtha into a brown soap that collects round the globule.

On the concrete oils (tallow, spermaceti, wax, for instance), when heated, it acts slowly, coaly matter is deposited, a little gas* is evolved, and a soap is formed; but in these cases it is necessary that a large quantity of the oil be employed. On the fluid fixed oils it produces the same effects, but more slowly.

By heat likewise it rapidly decomposes the volatile oils; alkali is formed, a small quantity of gas is evolved, and charcoal is deposited.

When the basis of potash is thrown into camphor in fusion, the camphor soon becomes blackened, no gas is liberated in the process of decomposition, and a saponaceous compound is formed; which seems to shew that camphor contains more oxygen than the volatile oils.

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* When a globule of the basis of potash is introduced into any of the fixed oils heated, the first product is pure hydrogen, which arises from the decomposition of the water absorbed by the crust of potash during the exposure to the atmosphere. The gas evolved, when the globule is freed from this crust, I have found to be carbonated hydrogen requiring more than an equal bulk of oxygen gas for its complete saturation by explosion. I have made a great number of experiments, which it would be foreign to the object of this lecture to give in minute detail, on the agencies of the basis of potash on the oils. Some anomalies occurred which led to the inquiry, and the result was perfectly conclusive. Olive oil, oil of turpentine, and naphtha when decomposed by heat, exhibited as products different portions of charcoal, heavy inflammable gas, empyreumatic oily matter, and water, so that the existence of oxygen in them was fully proved; and accurate indications of the proportions of their elements might be gained by their decomposition by the basis of potash. Naphtha of all furnished least water and carbonic acid, and oil of turpentine the most.

The basis of potash readily reduces metallic oxides when heated in contact with them. When a small quantity of the oxide of iron was heated with it, to a temperature approaching its point of distillation, there was a vivid action; alkali and grey metallic particles, which dissolved with effervescence in muriatic acid, appeared. The oxides of lead and the oxides of tin were revived still more rapidly; and when the basis of potash was in excess, an alloy was formed with the revived metal.

In consequence of this property, the basis of potash readily decomposes flint glass and green glass, by a gentle heat; alkali is immediately formed by oxygen from the oxides, which dissolves the glass, and a new surface is soon exposed to the agent.

At a red heat, even the purest glass is altered by the basis of potash: the oxygen in the alkali of the glass seems to be divided between the two bases, the basis of potash and the alkaline basis in the glass, and oxides, in the first degree of oxigenation, are the result.

When the basis of potash is heated in tubes made of plate glass filled with the vapour of naphtha, it first acts upon the small quantity of the oxides of cobalt and manganese in the interior surface of the glass, and a portion of alkali is formed. As the heat approaches to redness, it begins to rise in vapour, and condenses in the colder parts of the tube; but at the point where the heat is strongest, a part of the vapour seems to penetrate the glass, rendering it of a deep red brown colour; and by repeatedly distilling and heating the substance in a close tube of this kind, it finally loses its metallic form, and a thick brown crust, which slowly decomposes water, and which combines with oxygen when exposed to air forming alkali, lines the interior of the tube, and in many parts is found penetrating through its substance.*

In my first experiments on the distillation of the basis of potash, I had great difficulty in accounting for these phenomena; but the knowledge of the substance it forms in its first degree of union with oxygen, afforded a satisfactory explanation.

V. *On the Properties and Nature of the Basis of Soda.*

The basis of soda, as I have already mentioned, is a solid at common temperatures. It is white, opaque, and when examined under a film of naphtha, has the lustre and general appearance of silver. It is exceedingly malleable, and is much softer than any of the common metallic substances. When pressed upon by a platina blade, with a small force, it spreads into thin leaves, and a globule of the $\frac{1}{10}$th or $\frac{1}{12}$th of an inch in diameter is easily spread over a surface of a quarter of an inch,† and this property does not diminish when it is cooled to 32° Fahrenheit.

It conducts electricity and heat in a similar manner to the basis of potash; and small globules of it inflame by the voltaic electrical spark, and burn with bright explosions.

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* This is the obvious explanation in the present state of our knowledge; but it is more than probable that the silex of the glass likewise suffers some change, and probably decomposition. This subject I hope to be able to resume on another occasion.

† Globules may be easily made to adhere and form one mass by strong pressure: so that the property of welding, which belongs to iron and platina at a white heat only, is possessed by this substance at common temperatures.

Its specific gravity is less than that of water. It swims in oil of sassafras of 1·096, water being 1, and sinks in naphtha of specific gravity ·861. This circumstance enabled me to ascertain the point with precision. I mixed together oil of sassafras and naphtha, which combine very perfectly, observing the proportions till I had composed a fluid, in which it remained at rest above or below; and this fluid consisted of nearly twelve parts naphtha, and five of oil of sassafras, which gives a specific gravity to that of water, nearly as nine to ten, or more accurately as ·9348 to 1.

The basis of soda has a much higher point of fusion than the basis of potash; its parts begin to lose their cohesion at about 120° Fahrenheit, and it is a perfect fluid at about 180°, so that it readily fuses under boiling naphtha.

I have not yet been able to ascertain at what degree of heat it is volatile; but it remains fixed in a state of ignition at the point of fusion of plate glass.

The chemical phenomena produced by the basis of soda, are analogous to those produced by the basis of potash; but with such characteristic differences as might be well expected.

When the basis of soda is exposed to the atmosphere, it immediately tarnishes, and by degrees becomes covered with a white crust, which deliquesces much more slowly than the substance which forms on the basis of potash. It proves, on minute examination, to be pure soda.

The basis of soda combines with oxygen slowly, and without luminous appearance at all common temperatures; and when heated, this combination becomes more rapid; but no light is emitted till it has acquired a temperature nearly that of ignition.

The flame that it produces in oxygen gas is white, and it sends forth bright sparks, occasioning a very beautiful effect; in common air, it burns with light of the colour of that produced during the combustion of charcoal, but much brighter.

The basis of soda when heated in hydrogen, seemed to have no action upon it. When introduced into oximuriatic acid gas, it burned vividly with numerous scintillations of a bright red colour. Saline matter was formed in this combustion, which, as might have been expected, proved to be muriate of soda.

Its operation upon water offers most satisfactory evidence of its nature. When thrown upon this fluid, it produces a violent effervescence, with a loud hissing noise; it combines with the oxygen of the water to form soda, which is dissolved, and its hydrogen is disengaged. In this operation there is no luminous appearance; and it seems probable that even in the nascent state hydrogen is incapable of combining with it.*

When the basis of soda is thrown into hot water, the decomposition is more violent, and in this case a few scintillations are generally observed at the surface of the fluid; but this is owing to small particles of the basis, which are thrown out of the water sufficiently heated, to burn in passing through the atmosphere. When, however, a globule is brought in contact with a small particle of water, or with moistened paper, the heat produced (there being no medium to carry it off rapidly) is usually sufficient for the ascension of the basis.

The basis of soda acts upon alcohol and ether precisely in a similar manner with the basis of potash. The water that they contain is decomposed; soda is rapidly formed, and hydrogen disengaged.

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* The more volatile metals only seem capable of uniting with hydrogen; a circumstance presenting an analogy.

The basis of soda, when thrown upon the strong acids, acts upon them with great energy. When nitrous acid is employed, a vivid inflammation is produced; with muriatic and sulphuric acid, there is much heat generated, but no light.

When plunged, by proper means, beneath the surface of the acids, it is rapidly oxigenated; soda is produced, and the other educts are similar to those generated by the action of the basis of potash.

With respect to the fixed and volatile oils and naphtha in their different states, there is a perfect coincidence between the effects of the two new substances, except in the difference of the appearances of the saponaceous compounds formed: those produced by the oxidation and combination of the basis of soda being of a darker colour, and apparently less soluble.

The basis of soda, in its degrees of oxidation, has precisely similar habits with the basis of potash.

When it is fused with dry soda, in certain quantities, there is a division of oxygen between the alkali and the base; and a deep brown fluid is produced, which becomes a dark grey solid on cooling, and which attracts oxygen from the air, or which decomposes water, and becomes soda.

The same body is often formed in the analytical processes of decomposition, and it is generated when the basis of soda is fused in tubes of the purest plate glass.

There is scarcely any difference in the visible phenomena of the agencies of the basis of soda, and that of potash on sulphur, phosphorus, and the metals.

It combines with sulphur in close vessels filled with the vapour of naphtha with great vividness, with light, heat, and often with explosion from the vaporization of a portion of sulphur, and the disengagement of sulphuretted hydrogen gas. The sulphuretted basis of soda is of a deep grey colour.

The phosphuret has the appearance of lead, and forms phosphate of soda by exposure to air, or by combustion.

The basis of soda in the quantity of $\frac{1}{40}$, renders mercury a fixed solid of the colour of silver, and the combination is attended with a considerable degree of heat.

It makes an alloy with tin, without changing its colour, and it acts upon lead and gold when heated. I have not examined its habitudes with any other metals, but in its state of alloy, it is soon converted into soda by exposure to air, or by the action of water, which it decomposes, with the evolution of hydrogen.

The amalgam of mercury and the basis of soda, seems to form triple compounds with other metals. I have tried iron and platina, which I am inclined to believe remain in combination with the mercury, when it is deprived of the new substance by exposure to air.

The amalgam of the basis of soda and mercury likewise combines with sulphur and forms a triple compound of a dark grey colour.

VI. *On the Proportions of the peculiar Bases and Oxygen in Potash and Soda.*

The facility of combustion of the bases of the alkalies, and the readiness with which they decomposed water, offered means fully adequate for determining the proportions of the ponderable constituent parts.

I shall mention the general methods of the experiments, and the results obtained by the different series, which approach as near to each other as can be expected in operations performed on such small quantities of materials.

For the process in oxygen gas, I employed glass tubes containing small trays made of thin leaves of silver or other noble metals, on which the substance to be burnt, after being accurately weighed or compared with a globule of mercury, equal in size,* was placed: the tube was small at one end, curved, and brought to a fine point, but suffered to remain open; and the other end was fitted to a tube communicating with a gasometer, from which the oxygen gas was introduced, for neither water nor mercury could be used for filling the apparatus. The oxygen gas was carried through the tube till it was found that the whole of the common air was expelled. The degree of its purity was ascertained by suffering a small quantity to pass into the mercurial apparatus. The lower orifice was then hermetically sealed by a spirit lamp, and the upper part drawn out and finally closed, when the aperture was so small, as to render the temperature employed incapable of materially influencing the volume of the gas; and when the whole arrangement was made, the combination was effected by applying heat to the glass in contact with the metallic tray.

In performing these experiments many difficulties occurred. When the flame of the lamp was immediately brought to play upon the glass, the combustion was very vivid, so as sometimes to break the tube; and the alkali generated partly rose in white fumes, which were deposited upon the glass.

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* When the globules were very small, the comparison with mercury, which may be quickly made by means of a micrometer, was generally employed as the means of ascertaining the weight: for in this case the globule could be immediately introduced into the tube, and the weight of mercury ascertained at leisure.

When the temperature was slowly raised, the bases of the alkalies acted upon the metallic tray and formed alloys, and in this state it was very difficult to combine them with their full proportion of oxygen; and glass alone could not be employed on account of its decomposition by the alkaline bases; and porcelain is so bad a conductor of heat, that it was not possible to raise it to the point required for the process, without softening the glass.

In all cases the globules of the alkaline bases were carefully freed from naphtha before they were introduced; of course a slight crust of alkali was formed before the combustion, but this could not materially affect the result; and when such a precaution was not used, an explosion generally took place from the evaporization and decomposition of the film of naphtha surrounding the globule.

After the combustion, the absorption of gas was ascertained, by opening the lower point of the tube under water or mercury. In some cases the purity of the residual air was ascertained, in others the alkali formed in the tray was weighed.

From several experiments on the synthesis of potash by combustion, I shall select two, which were made with every possible attention to accuracy, and under favourable circumstances, for a mean result.

In the first experiment 0·12 grains of the basis were employed. The combustion was made upon platina, and was rapid and complete; and the basis appeared to be perfectly saturated, as no disengagement of hydrogen took place when the platina tray was thrown into water. The oxygen gas absorbed equalled in volume 190 grain measures of quicksilver; barometer being at 29·6 inches, thermometer 62° Fahrenheit; and this reduced to a temperature of 60° Fahrenheit, and under a pressure equal to that indicated by 30 inches,[*] would become 186·67 measures, the weight of which would be about ·0184 grains troy;[†] but ·0184 : ·1384 : : 13·29 : 100; and according to this estimation 100 parts of potash will consist of 86·7 basis, and 13·3 oxygen nearly.

In the second experiment ·07 grains of the basis absorbed at temperature 63° of Fahrenheit, and under pressure equal to 30·1 barometer inches, a quantity of oxygen equal in volume to 121 grain measures of mercury, and the proper corrections being made as in the former case, this gas would weigh ·01189 grains.

But as ·07 + ·01189 = ·08189 : 07 : : 100 : 85·48 nearly, and 100 parts of potash will consist of 85·5 of basis and 14·5 of oxygen nearly. And the mean of the two experiments will be 86·1 of basis to 13·9 of oxygen for 100 parts.

In the most accurate experiment that I made on the combustion of the basis of soda, ·08 parts of the basis absorbed a quantity of oxygen equal to 206 grain measures of mercury; the thermometer being at 56° Fahrenheit; and the barometer at 29·4; and this quantity, the corrections being made as before for the mean temperature and pressure, equals about ·02 of oxygen.

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[*] In the correction for temperature, the estimations of Dalton and Gay Lussac are taken, which make gases expand about $\frac{1}{480}$ of the primitive volume for every degree of Fahrenheit.

[†] From experiments that I made in 1799, on the specific gravity of oxygen gas, it would appear that its weight is to that of water as 1 to 743, and to that of quicksilver as 1 to 10142, Researches Chem. and Phil. p. 9; and with this estimation, that deducible from the late accurate researches of Messrs. Allen and Pepy's on the Combustion of the Diamond almost precisely agrees, Phil. Trans. 1807, p. 275.

And as ·08 + ·02 = ·10 : ·08, : : 100 : 80, and 100 parts of soda according to this estimation will consist of 80 basis to 20 of oxygen.

In all cases of slow combustion, in which the alkalies were not carried out of the tray, I found a considerable increase of weight, but as it was impossible to weigh them except in the atmosphere, the moisture attracted rendered the results doubtful; and the proportions from the weight of the oxygen absorbed are more to be depended on. In the experiments in which the processes of weighing were most speedily performed, and in which no alkali adhered to the tube, the basis of potash gained nearly 2 parts for 10, and that of soda between 3 and 4 parts.

The results of the decomposition of water by the basis of the alkalies were much more readily and perfectly obtained than those of their combustion.

To check the rapidity of the process, and, in the case of potash, to prevent any of the basis from being dissolved, I employed the amalgams with mercury. I used a known weight of the basis, and made the amalgams under naphtha, using about two parts of mercury in volume to one of basis.

In the first instances I placed the amalgams under tubes filled with naphtha, and inverted in glasses of naphtha, and slowly admitted water to the amalgam at the bottom of the glass; but this precaution I soon found unnecessary, for the action of the water was not so intense but that the hydrogen gas could be wholly collected.

I shall give an account of the most accurate experiments made on the decomposition of water by the bases of potash and soda.

In an experiment on the basis of potash conducted with every attention that I could pay to the minutiæ of the operations, hydrogen gas, equal in volume to 298 grains of mercury, were disengaged by the action of ·08 grains of the basis of potash which had been amalgamated with about 3 grains of mercury. The thermometer at the end of the process indicated a temperature of 56° Fahrenheit, and the barometer an atmospheric pressure equal to 29·6 inches.

Now this quantity of hydrogen* would require for its combustion a volume of oxygen gas about equal to that occupied by 154·9 grains of mercury, which gives the weight of oxygen required to saturate the ·08 grains of the basis of potash at the mean temperature and pressure nearly ·0151 grains. And ·08 + ·0151 = ·0951 : ·08 : : 100 : 84·1 nearly.

And according to these indications 100 parts of potash consist of about 84 basis and 16 oxygen.

In an experiment on the decomposition of water by the basis of soda, the mercury in the barometer standing at 30·4 inches, and in the thermometer at 52° Fahrenheit, the volume of hydrogen gas evolved by the action of ·054 grains of basis equalled that of 326 grains of quicksilver. Now this at the mean temperature and pressure would require for its conversion into water, ·0172 of oxygen, and ·054 + ·0172 = ·0712 : ·054 : : 100 : 76 nearly; and according to these indications, 100 parts of soda consist of nearly 76 basis, and 24 oxygen.

In another experiment made with very great care, ·052 of the basis of soda were used; the mercury in the barometer was at 29·9 inches, and that in the thermometer at 58° Fahrenheit. The volume of hydrogen evolved was equal to that of 302 grains of mercury; which would demand for its saturation by combustion, at the mean temperature and pressure ·01549 grains of oxygen; and 100 parts of soda, according to this proportion, would consist nearly of 77 basis, and 23 oxygen.

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* Researches Chem. and Phil. p. 287. [Vol. III. p. 170.]

The experiments which have been just detailed, are those in which the largest quantities of materials were employed; I have compared their results, however, with the results of several others, in which the decomposition of water was performed with great care, but in which the proportion of the bases was still more minute: the largest quantity of oxygen indicated by these experiments was, for potash 17, and for soda 26 parts in 100, and the smallest 13, and 19; and comparing all the estimations, it will probably be a good approximation to the truth, to consider potash as composed of about 6 parts basis and 1 of oxygen; and soda, as consisting of 7 basis and 2 oxygen.*

VII. *Some general Observations on the Relations of the Bases of Potash and Soda to other Bodies.*

Should the bases of potash and soda be called metals? The greater number of philosophical persons to whom this question has been put, have answered in the affirmative. They agree with metals in opacity, lustre, malleability, conducting powers as to heat and electricity, and in their qualities of chemical combination.

Their low specific gravity does not appear a sufficient reason for making them a new class; for amongst the metals themselves there are remarkable differences in this respect, platina being nearly four times as heavy as tellurium;* and in the philosophical division of the classes of bodies, the analogy between the greater number of properties must always be the foundation of arrangement.†

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* [These estimates of the proportion of oxygen in potassa and soda are remarkable approximations, considering the very minute quantities submitted to experiment. For the author's later results, operating on larger quantities, vide Vol. IV. p. 240.]

On this idea, in naming the bases of potash and soda, it will be proper to adopt the termination which, by common consent, has been applied to other newly discovered metals, and which, though originally Latin, is now naturalized in our language.

Potasium and sodium are the names by which I have ventured to call the two new substances: and whatever changes of theory, with regard to the composition of bodies, may hereafter take place, these terms can scarcely express an error; for they may be considered as implying simply the metals produced from potash and soda. I have consulted with many of the most eminent scientific persons in this country, upon the methods of derivation, and the' one I have adopted has been the one most generally approved. It is perhaps more significant than elegant. But it was not possible to found names upon specific properties not common to both; and though a name for the basis of soda might have been borrowed from the Greek, yet an analogous one could not have been applied to that of potash, for the ancients do not seem to have distinguished between the two alkalies.

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* Tellurium is not much more than six times as heavy as the basis of soda. There is great reason to believe that bodies of a similar chemical nature to the bases of potash and soda will be found of intermediate specific gravities between them and the lightest of the common metals. Of this subject, I shall treat again in the text in some of the following pages.

† [The above paragraph and the note at the bottom of the following page afford a striking example of philosophical clear-sightedness and precision, especially when contrasted with the different views of the bases of potassa and soda taken by other inquirers, and those chemists of the first eminence, as MM. Gay Lussac and Thenard, Dr. Dalton, and M. Berzelius,—views which were strenuously defended for a time; were firmly opposed by the author, chiefly on the above data, as will appear in the sequel,—and were ultimately relinquished.]

The more caution is necessary in avoiding any theoretical expression in the terms, because the new electrochemical phenomena that are daily becoming disclosed, seem distinctly to shew that the mature time for a complete generalization of chemical facts is yet far distant; and though, in the explanations of the various results of experiments that have been detailed, the antiphlogistic solution of the phenomena has been uniformly adopted, yet the motive for employing it has been rather a sense of its beauty and precision, than a conviction of its permanency and truth.

The discovery of the agencies of the gases destroyed the hypothesis of Stahl. The knowledge of the powers and effects of the etherial substances may at a future time possibly act a similar part with regard to the more refined and ingenious hypothesis of Lavoisier; but in the present state of our knowledge, it appears the best approximation that has been made to a perfect logic of chemistry.

Whatever future changes may take place in theory, there seems however every reason to believe that the metallic bases of the alkalies, and the common metals, will stand in the same arrangement of substances; and as yet we have no good reasons for assuming the compound nature of this class of bodies.*

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* A phlogistic chemical theory might certainly be defended, on the idea that the metals are compounds of certain unknown bases with the

The experiments in which it is said that alkalies, metallic oxides, and earths may be formed from air and water alone, in processes of vegetation, have been always made in an inconclusive manner;* for distilled water, as I have endeavoured to show,† may contain both saline and metallic impregnations; and the free atmosphere almost constantly holds in mechanical suspension solid substances of various kinds.

In the common processes of nature, all the products of living beings may be easily conceived to be elicited from known combinations of matter. The compounds of iron, of the alkalies, and earths, with mineral acids, generally abound in soils. From the decomposition of

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same matter as that existing in hydrogen; and the metallic oxides, alkalies and acids compounds of the same bases with water;—but in this theory more unknown principles would be assumed than in the generally received theory. It would be less elegant and less distinct. In my first experiments on the distillation of the basis of potash finding hydrogen generally produced, I was led to compare the phlogistic hypothesis with the new facts, and I found it fully adequate to the explanation. More delicate researches however afterwards proved that in the cases when inflammable gases appeared, water, or some body in which hydrogen is admitted to exist, was present.

* The explanation of Van Helmont of his fact of the production of earth in the growth of the willow, was completely overturned by the researches of Woodward. Phil. Trans. vol. xxi. p. 193.

The conclusions which M. Braconnot has very lately drawn from his ingenious experiments, Annales de Chemie, Fevrier 1807, page 187, are rendered of little avail in consequence of the circumstances stated in the text. In the only case of vegetation in which the free atmosphere was excluded, the seeds grew in white sand, which is stated to have been purified by washing in muriatic acid; but such a process was insufficient to deprive it of substances which might afford carbon, or various inflammable matters. Carbonaceous matter exists in several stones which afford a whitish or greyish powder; and when in a stone, the quantity of carbonate of lime is very small in proportion to the other earthy ingredients, it is scarcely acted on by acids.

† Bakerian Lecture, 1806, page 8.

basaltic, porphyritic,* and granitic rocks, there is a constant supply of earthy alkaline and ferruginous materials to the surface of the earth. In the sap of all plants that have been examined, certain neutrosaline compounds, containing potash, or soda, or iron, have been found. From plants they may be supplied to animals. And the chemical tendency of organization seems to be rather to combine substances into more complicated and diversified arrangements, than to reduce them into simple elements.

VIII. *On the Nature of Ammonia and Alkaline Bodies in general; with Observations on some prospects of Discovery offered by the preceding Facts.*

Ammonia is a substance, the chemical composition of which has always been considered of late years as most perfectly ascertained, and the apparent conversion of it into hydrogen and nitrogen, in the experiments of Scheele, Priestley, and the more refined and accurate experiments of Berthollet, had left no doubt of its nature in the minds of the most enlightened chemists.

All new facts must be accompanied however by a train of analogies, and often by suspicions with regard to the accuracy of former conclusions. As the two fixed alkalies contain a small quantity of oxygen united to peculiar bases, may not the volatile alkali likewise contain it? was a query which soon occurred to me in the course of inquiry; and in perusing the accounts of the various experiments made on the subject, some of which I had carefully repeated, I saw no reason to consider the circumstance as impossible. For supposing hydrogen and nitrogen to exist in combination with oxygen in low proportion, this last principle might easily disappear in the analytical experiments of decomposition by heat and electricity, in water deposited upon the vessels employed or dissolved in the gases produced.

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* In the year 1804, for a particular purpose of geological inquiry, I made an analysis of the porcelain clay of St. Stevens, in Cornwall, which results from the decomposition of the feldspar of fine-grained granite. I could not detect in it the smallest quantity of alkali. In making some experiments on specimens of the undecompounded rock taken from beneath the surface, there were evident indications of the presence of a fixed alkali, which seemed to be potash. So that it is very probable that the decomposition depends on the operation of water and the carbonic acid of the atmosphere on the alkali forming a constituent part of the crystalline matter of the feldspar, which may disintegrate from being deprived of it.

Of the existence of oxygen in volatile alkali I soon satisfied myself. When charcoal carefully burnt and freed from moisture was ignited by the Voltaic battery of the power of 250 of 6 and 4 inches square, in a small quantity of very pure ammoniacal gas:* a great expansion of the aëriform matter took place, and a white substance formed, which collected on the sides of the glass tube employed in the process; and this matter, exposed to the action of diluted muriatic acid, effervesced, so that it was probably carbonate of ammonia.

A process of another kind offered still more decisive results. In this the two mercurial gasometers of the invention of Mr. Pepys, described in No. xiv. of the Phil. Trans. for 1807, were used with the same apparatus, as that employed by Messrs. Allen and Pepys for the combustion of the diamond, and these gentlemen kindly assisted in the experiment.

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* The apparatus in which this experiment was made is described in page 214 Journal of the Royal Institution. [Vol. II. p. 220.] The gas was confined by mercury which had been previously boiled to expel any moisture that might adhere to it. The ammonia had been exposed to the action of dry pure potash, and a portion of it equal in volume to 10980 grains of mercury, when acted on by distilled water, left a residuum equal to 9 grains of mercury only. So that the gas, there is every reason to believe, contained no foreign aëriform matter; for even the minute residuum may be accounted for by supposing it derived from air dissolved in the water.

Very pure ammoniacal gas was passed over iron wire ignited in a platina tube, and two curved glass tubes were so arranged as to be inserted into a freezing mixture; and through one of these tubes the gas entered into the platina tube, and through the other, it passed from the platina tube into the airholder arranged for its reception.

The temperature of the atmosphere was 55°; but it was observed that no sensible quantity of water was deposited in the cooled glass tube transmitting the unaltered ammonia, but in that receiving it after its exposure to heat, moisture was very distinct, and the gas appeared in the airholder densely clouded.

This circumstance seems distinctly to prove the formation of water in this operation for the decomposition of ammonia; unless indeed it be asserted that the hydrogen and nitrogen gases evolved hold less water in solution or suspension than the ammonia decomposed, an idea strongly opposed by the conclusions of Mr. Dalton,[*] and the experiments of Messrs. Desormes and Clement.[†]

After the gas had been passed several times through the ignited tube from one gasometer to the other, the results were examined. The iron wire became converted superficially into oxide, and had gained in weight $\frac{44}{100}$ parts of a grain, about $\frac{4}{10}$ of a grain of water were collected from the cooled glass tubes by means of filtrating paper, and 33·8 cubic inches of gas were expanded into 55·3 cubic inches, and by detonation with oxygen it was found that the hydrogen gas in these was to the nitrogen as 3·2 to 1 in volume.

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[*] Manchester Memoirs, vol. v. part ii. p. 535. 1785.

[†] Annales de Chemie, vol. xlii. p. 125.

It will be useless to enter into the more minute details of this experiment, as no perfectly accurate data for proportions can be gained from them; for the whole of the ammonia was not decomposed, and as the gas had been prepared by being sent from a heated mixture of sal ammoniac and quicklime, into the airholder, it was possible that some solution of ammonia might have been deposited, which, by giving out new gas during the operation, would increase the absolute quantity of the material acted upon.

In examining the results of M. Berthollet's * elaborate experiments on the decomposition of ammonia by electricity, I was surprised to find that the weight of the hydrogen and nitrogen produced, rather exceeded than fell short of that of the ammonia considered as decomposed, which was evidently contradictory to the idea of its containing oxygen. This circumstance, as well as the want of coincidence between the results and those of Priestley and Van Marum on the same subject, induced me to repeat the process of the electrization of ammonia, and I soon found that the quantities of the products in their relations to the apparent quantity of gas destroyed were influenced by many different causes.

Ammonia procured over dry mercury from a mixture of dry lime and muriate of ammonia, I found deposited moisture upon the sides of the vessel in which it was collected, and in passing the gas into the tube for

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* Mémoires de l'Academie, 1782. p. 324.

electrization, it was not easy to avoid introducing some of this moisture, which must have been a saturated solution of ammonia, at the same time.

In my first trials made upon gas, passed immediately from the vessel in which it had been collected into the apparatus, I found the expansion of 1 of ammonia vary in different instances from 2·8 to 2·2 measures, but the proportions of the nitrogen and hydrogen appeared uniform, as determined by detonation of the mixed gas with oxygen, and nearly as 1 to 3 in volume.

To exclude free moisture entirely, I carefully prepared ammonia in a mercurial airholder, and after it had been some hours at rest, passed a quantity of it into the tube for decomposition, which had been filled with dry mercury. In this case 50 parts became 103 parts by electrization, and there was still reason to suspect sources of error.

I had used iron wires not perfectly free from rust, for taking the spark, and a black film from the mercury appeared on the sides of the tube. It was probable that some ammonia had been absorbed by the metallic oxides both upon the iron and the mercury, which might again have been given out in the progress of the operation.

I now used recently distilled mercury, which did not leave the slightest film on the glass tube, and wires of platina. The ammonia had been exposed to dry caustic potash, and proved to be equally pure with that mentioned in page 93. 60 measures of it, each equal to a grain of water, were electrized till no farther expansion could be produced, the gas filled a space equal to that occupied by 108 grains of water. The thermometer in this experiment was at 56°, and the barometer at 30·1 inches. The wire of platina transmitting the spark was slightly tarnished.* The 108 measures of gas carefully analyzed, were found to consist of 80 measures in volume of hydrogen, and 28 measures of nitrogen.

The results of an experiment that I made in 1799,† give the weight of 100 cubic inches of ammonia, as 18·18 grains at the mean temperature and pressure. I had reasons however for suspecting that this estimation might be somewhat too low, and on mentioning the circumstance to Messrs. Allen and Pepys, they kindly undertook the examination of the subject, and Mr. Allen soon furnished me with the following data. "In the first experiment 21 cubic inches of ammonia weighed 4·05 grains; in a second experiment the same quantity weighed 5·06 grains, barometer 30·65, thermometer 54° Fahrenheit."

Now if the corrections for temperature and pressure be made for these estimations, and a mean taken, 100 cubic inches of ammonia will weigh 18·67 grains, barometer being at 30, and thermometer at 60° Fahrenheit; and if the quantity used in the experiment of decomposition be calculated upon as cubic inches, 60 will weigh 11·2 grains. But the hydrogen gas evolved equal to 80 will weigh 1·93‡ grains, and the nitrogen equal to 28,§ 8·3. And $11{\cdot}2$ grains $-1{\cdot}9+8{\cdot}3=10{\cdot}2$, or $11{\cdot}2-10{\cdot}2.=1$, all the estimations being made according to the standard temperature and pressure.

So that in this experiment on the decomposition of ammonia, the weight of the gases evolved is less by nearly $\frac{1}{11}$ than that of the ammonia employed; and this loss can only be ascribed to the existence of oxygen in the alkali; part of which probably combined with the platina wires employed for electrization, and part with hydrogen.

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* This most probably was owing to oxidation. When platina is made positive in the Voltaic circuit in contact with solution of ammonia, it is rapidly corroded; this is an analogous instance.

† Researches Chem. and Phil. p. 62. [Vol. III. p. 41.]

‡ Lavoisier's Elements, p. 569. A cubical inch of hydrogen is considered as weighing ·0239.

§ Researches Chem. and Phil. p. 9. From my experiments 100 cubical inches of nitrogen weigh at the standard temperature and pressure 29·6 grains.

After these ideas the oxygen in ammonia cannot well be estimated at less than 7 or 8 parts in the hundred; and it possibly exists in a larger proportion, as the gases evolved may contain more water than the gas decomposed, which of course would increase their volume and their absolute weight.*

In supposing ammonia a triple compound of nitrogen, hydrogen, and oxygen, it is no less easy to give a rational account of the phenomena of its production and decomposition, than in adopting the generally received hypothesis of its composition.

Oxygen, hydrogen, and nitrogen are always present in cases in which volatile alkali is formed; and it usually appears during the decomposition of bodies in which oxygen is loosely attached, as in that of the compounds of oxygen and nitrogen dissolved in water.

At common temperatures under favourable circumstances, the three elements may be conceived capable of combining and of remaining in union: but at the heat of ignition the affinity of hydrogen for oxygen prevails over the complex attraction, water is formed, and hydrogen and nitrogen are evolved; and according to these conclusions, ammonia will bear the same relations to the fixed alkalies, as the vegetable acids with compound bases do to the mineral ones with simple bases.*

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* In the present state of our knowledge, perfectly correct data for proportions cannot probably be gained in any experiments on the decomposition of ammonia, as it seems impossible to ascertain the absolute quantity of water in this gas, for electrization, according to Dr. Henry's ingenious researches, offers the only means known of ascertaining the quantity of water in gases.

Oxygen then may be considered as existing in, and as forming, an element in all the true alkalies; and the principle of acidity of the French nomenclature, might now likewise be called the principle of alkalescence.

From analogy alone it is reasonable to expect that the alkaline earths are compounds of a similar nature to the fixed alkalies, peculiar highly combustible metallic bases united to oxygen. I have tried some experiments upon barytes and strontites; and they go far towards proving that this must be the case. When barytes and strontites, moistened with water, were acted upon by the power of the battery of 250 of 4 and 6, there was a vivid action and a brilliant light at both points of communication, and an inflammation at the negative point.

In these cases the water might possibly have interfered. Other experiments gave however more distinct results.

Barytes and strontites, even when heated to intense whiteness, in the electrical circuit by a flame supported by oxygen gas, are non-conductors; but by means of combination with a very small quantity of boracic acid, they become conductors; and in this case inflammable matter, which burns with a deep red light in each instance, is produced from them at the negative surface. The high temperature has prevented the success of attempts to collect this substance; but there is much reason to believe that it is the basis of the alkaline earth employed.

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* [The above results apparently proving the existence of oxygen in ammonia were not confirmed by ulterior and more careful inquiry, as will appear in the after papers. Of all the subjects of research which ever engaged the attention of the author, the nature of ammonia perplexed him most, and exercised in a remarkable manner his patience, his industry, and his skill. Persons not acquainted with the labours of original research, and the perseverance requisite to solve obscure problems experimentally, would have difficulty in imagining even, the pains he took, the time he spent, the vast number of experiments he made to resolve his doubts, on the question of the nature of ammonia; the details which he published, and which will be found in the subsequent papers, give but an imperfect idea of the varied, numerous, and laborious experiments he engaged in on this very curious, important and difficult subject, which still, in spite of all the labour expended and ingenuity exercised, continues to be an unsolved problem.]

Barytes and strontites have the strongest relations to the fixed alkalies of any of the earthy bodies;* but there is a chain of resemblances, through lime, magnesia, glucina, alumina, and silex. And by the agencies of batteries sufficiently strong, and by the application of proper circumstances, there is no small reason to hope, that even these refractory bodies will yield their elements to the methods of analysis by electrical attraction and repulsion.

In the electrical circuit we have a regular series of powers of decomposition, from an intensity of action so feeble as scarcely to destroy the weakest affinity existing between the parts of a saline neutral compound,† to one sufficiently energetic to separate elements of the strongest degree of union, in bodies undecomposed under other circumstances.

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* The similarity between the properties of earths and metallic oxides, was noticed in the early periods of chemistry. The poisonous nature of barytes, and the great specific gravity of this substance as well as of strontites, led Lavoisier to the conjecture that they were of a metallic nature. That metals existed in the fixed alkalies seems however never to have been suspected. From their analogy to ammonia, nitrogen and hydrogen have been supposed to be amongst their elements. It is singular, with regard to this class of bodies, that those most unlike metallic oxides are the first which have been demonstrated to be such.

† [Such as the author afterwards availed himself of, in his capital application of a low electro-chemical power, to protect from rapid decay the copper sheathing of ships, hardly less wonderful in operation than the cooling agency of wire gauze, the principle of the safety lamp.]

When the powers are feeble, acids and alkalies, and acids and metallic oxides, merely separate from each other; when they are increased to a certain degree, the common metallic oxides and the compound acids are decomposed; and by means still more exalted, the alkalies yield their elements. And as far as our knowledge of the composition of bodies extends, all substances attracted by positive electricity, are oxygen, or such as contain oxygen in excess; and all that are attracted by negative electricity, are pure combustibles, or such as consist chiefly of combustible matter.

The idea of muriatic acid, fluoric acid, and boracic acid containing oxygen, is highly strengthened by these facts. And the general principle confirms the conjecture just stated concerning the nature of the earths.

In the electrization of boracic acid moistened with water, I find that a dark coloured combustible matter is evolved at the negative surface; but the researches upon the alkalies have prevented me from pursuing this fact, which seems however to indicate a decomposition.

Muriatic acid and fluoric acid in their gaseous state are non-conductors: and as there is every reason to believe that their bases have a stronger attraction for oxygen than water, there can be little hope of decomposing them in their aqueous solutions, even by the highest powers. In the electrization of some of their combinations there is however a probability of success.

An immense variety of objects of research is presented in the powers and affinities of the new metals produced from the alkalies.*

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* [In the Memoir of the Life of the Author, it has been mentioned how

In themselves they will undoubtedly prove powerful agents for analysis; and having an affinity for oxygen stronger than any other known substances, they may possibly supersede the application of electricity to some of the undecompounded bodies.

The basis of potash I find oxidates in carbonic acid and decomposes it, and produces charcoal when heated in contact with carbonate of lime. It likewise oxidates in muriatic acid; but I have had no opportunity of making the experiment with sufficient precision to ascertain the results.

In sciences kindred to chemistry, the knowledge of the nature of the alkalies, and the analogies arising in consequence, will open many new views; they may lead to the solution of many problems in geology, and shew that agents may have operated in the formation of rocks and earths which have not hitherto been suspected to exist.

It would be easy to pursue the speculative part of this inquiry to a great extent, but I shall refrain from so occupying the time of the Society, as the tenour of my object in this lecture has not been to state hypotheses, but to bring forward a new series of facts.

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his labours at this interesting moment were interrupted by dangerous illness of several weeks' duration. On his recovery, he found the subjects he had been investigating, seized on by MM. Gay Lussac, and Thenard, rather in the manner and feeling of contending generals intent on conquest, than of philosophical inquirers, members of the common republic of science. This proceeding, it cannot be concealed, annoyed him at the time; especially as there was often a want of reference on their part to his previous labours: and it necessarily had the effect of hurrying on his researches, as will be perceived in many of the following papers.]

III.

ELECTRO-CHEMICAL RESEARCHES ON THE DECOMPOSITION OF THE EARTHS; WITH OBSERVATIONS ON THE METALS OBTAINED FROM THE ALKALINE EARTHS, AND ON THE AMALGAM PROCURED FROM AMMONIA.*

I. *Introduction.*

In the Philosophical Transactions for 1807, Part I. and 1808, Part I.† I have detailed the general methods of decomposition by electricity, and stated various new facts obtained in consequence of the application of them.

The results of the experiments on potash and soda, as I stated in my last communication to the Society, afforded me the strongest hopes of being able to effect the decomposition both of the alkaline and common earths; and the phenomena obtained in the first imperfect trials made upon those bodies countenanced the ideas that had obtained from the earliest periods of chemistry, of their being metallic in their nature.‡

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* [From the Philosophical Transactions; read before the Royal Society, June 30, 1808.]

† [The preceding Bakerian Lectures.]

‡ Beccher is the first chemist, as far as my reading informs me, who distinctly pointed out the relations of metals to earthy substances. See Phys. subt. Lipsiæ, 4to. p. 61. He was followed by Stahl, who has given the doctrine a more perfect form. Beccher's idea was that of an universal elementary earth, which, by uniting to an inflammable earth,

Many difficulties however occurred in the way of obtaining complete evidence on this subject: and the pursuit of the inquiry has required much labour and a considerable devotion of time, and has demanded more

produced all the metals, and under other modifications formed stones. Stahl admitted distinct earths which he supposed might be converted into metals by combining with phlogiston; see Stahl Fundament. Chym. p. 9. 4to. and Conspect. Chem. 1. 77. 4to.—Neuman gives an account of an elaborate series of unsuccessful experiments which he made to obtain a metal from quicklime. Lewi's Neuman's Chem. Works, 2d edit. vol. i. p. 15. The earlier English chemical philosophers seem to have adopted the opinion of the possibility of the production of metals from common earthy substances; see Boyle, vol. i. 4to. p. 564, and Grew, Anatomy of Plants, lec. ii. p. 242. But these notions were founded upon a kind of alchemical hypothesis of a general power in nature of transmuting one species of matter into another. Towards the end of the last century the doctrine was advanced in a more philosophical form; Bergman suspected barytes to be a metallic calx, Præf. Sciagrap. Reg. Min. & Opusc. iv. 212. Baron supported the idea of the probability of alumine being a metallic substance, see Annales de Chimie, vol. x. p. 257.—Lavoisier extended these notions, by supposing the other earths metallic oxides. Elements, 2d edit. Kerr's translation, p. 217. The general inquiry was closed by the assertion of Tondi and Ruprecht, that the earths might be reduced by charcoal; and the accurate researches of Klaproth and Savaresi, who proved by the most decisive experiments, that the metals taken for the bases of the earths were phosphurets of iron, obtained from the bone ashes and other materials employed in the experiment, Annales de Chimie, vol. viii. p. 18. and vol. x. p. 257. 275. Amidst all these hypotheses, potash and soda were never considered as metallic in their nature; Lavoisier supposed them to contain azote; nor at that time were there any analogies to lead that acute philosopher to a happier conjecture.

[The author in a MS. Lecture, delivered at the Royal Institution, describing the train of inquiry, which terminated in the decomposition of the fixed alkalies, remarks, that on entering upon it, "The suspicion strongest in my mind was, that it (potassa) might consist of phosphorus, or sulphur united to nitrogen; for, as the volatile alkali was regarded as composed of an extremely light inflammable body, hydrogen united to nitrogen, I conceived that *phosphorus* and *sulphur*, much denser bodies, might produce denser alkaline matter; and as there were no *known* combinations of these with *nitrogen*, it was probable that there might be unknown combinations."]

refined and complicated processes than those which had succeeded with the fixed alkalies.

The earths like the fixed alkalies are non-conductors of electricity; but the fixed alkalies become conducting by fusion: the infusible nature of the earths, however, rendered it impossible to operate upon them in this state: the strong affinity of their bases for oxygen, made it unavailing to act upon them in solution in water; and the only methods that proved successful, were those of operating upon them by electricity in some of their combinations, or of combining them at the moment of their decomposition by electricity, in metallic alloys, so as to obtain evidences of their nature and properties.

I delayed for some time laying an account of many of the principal results which I obtained before the Society, in the hopes of being able to render them more distinct and satisfactory; but finding that for this end a more powerful battery, and more perfect apparatus than I have a prospect of seeing very soon constructed, will be required, I have ventured to bring forwards the investigation in its present imperfect state; and I shall prefer the imputation of having published unfinished labours, to that of having concealed any new facts from the scientific world, which may tend to assist the progress of chemical knowledge.

II. *Methods employed for decomposing the Alkaline Earths.*

Barytes, strontites, and lime, slightly moistened, were electrified by iron wires under naphtha, by the same methods, and with the same powers as those employed for the decomposition* of the fixed alkalies. In these cases, gas was copiously evolved, which was inflammable; and the earths where in contact with the negative metallic wires became dark coloured, and exhibited small points having a metallic lustre, which, when exposed to air, gradually became white; they became white likewise when plunged under water, and when examined in this experiment by a magnifier, a greenish powder seemed to separate from them, and small globules of gas were disengaged.

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* See page 60.

In these cases there was great reason to believe that the earths had been decomposed; and that their bases had combined with the iron, so as to form alloys decomposable by the oxygen of air or water; but the indistinctness of the effect, and the complicated circumstances required for it, were such as to compel me to form other plans of operation.

The strong attraction of potassium for oxygen, induced me to try whether this body might not detach the oxygen from the earths, in the same manner as charcoal decomposes the common metallic oxides.

I heated potassium in contact with dry pure lime, barytes, strontites, and magnesia, in tubes of plate glass; but as I was obliged to use very small quantities,* and as I could not raise the heat to ignition without fusing the glass, I obtained in this way no good results. The potassium appeared to act upon the earths and on the glass, and dark brown substances were obtained, which evolved gas from water; but no distinct metallic globules could be procured: from these circumstances, and other like circumstances, it seemed probable, that though potassium may partially de-oxygenate the earths, yet its affinity for oxygen, at least at the temperature which I employed, is not sufficient to effect their decomposition.

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* [Procured by means of the Voltaic battery; the chemical method of obtaining the metal was not then known in this country; vide note by the author towards the end of this paper.]

I made mixtures of dry potash in excess and dry barytes, lime, strontites, and magnesia, brought them into fusion, and acted upon them in the voltaic circuit in the same manner as that I employed for obtaining the metals of the alkalies. My hopes were, that the potassium, and the metals of the earths might be deoxigenated at the same time, and enter into combination in alloy.

In this way of operating, the results were more distinct than in the last: metallic substances appeared less fusible than potassium, which burnt the instant after they had formed, and which by burning produced a mixture of potash and the earth employed; I endeavoured to form them under naphtha, but without much success. To produce the result at all, required a charge by the action of nitric acid, which the state of the batteries did not permit me often to employ;* and the metal was generated only in very minute films, which could not be detached by fusion, and which were instantly destroyed by exposure to air.

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* The power of this combination, though it consisted of one hundred plates of copper and zinc of six inches, and one hundred and fifty of four inches, at this time was not more than equal to that of a newly constructed apparatus of one hundred and fifty, of four inches. It had been made for the demonstrations in the Theatre of the Royal Institution in 1803; and since that time had been constantly employed in the annual courses of Lectures, and had served in different parts, for the numerous experiments on the decomposition of bodies by electricity, detailed in the Bakerian Lectures for 1806 and 1807, and a number of the plates were destroyed by corrosion. I mention these circumstances, because many chemists have been deterred from pursuing experiments on the decomposition of the alkalies and the earths, under the idea that a very powerful combination was required for the effect. This, however, is far from being the case; all the experiments detailed in the text may

I had found in my researches upon potassium, that when a mixture of potash and the oxide of mercury, tin, or lead, was electrified in the Voltaic circuit, the decomposition was very rapid, and an amalgam, or an alloy of potassium was obtained; the attraction between the common metals and the potassium apparently accelerating the separation of the oxygen.

The idea that a similar kind of action might assist the decomposition of the alkaline earths, induced me to electrify mixtures of these bodies and the oxide of tin, of iron, of lead, of silver, and of mercury; and these operations were far more satisfactory than any of the others.

A mixture of two-thirds of barytes and one-third of oxide of silver very slightly moistened was electrified by iron wires; an effervescence took place at both points of contact, and a minute quantity of a substance, possessing the whiteness of silver, formed at the negative point. When the iron wire to which this substance adhered was plunged into water containing a little alum in solution, gas was disengaged, which proved to be hydrogen; and white clouds which were found to be sulphate of barytes, descended from the point of the wire.

A mixture of barytes and red oxide of mercury, in the same proportions, was electrified in the same manner. A small mass of solid amalgam adhered to the negative wire, which evidently contained a substance, that produced barytes by exposure to air, with the

be repeated by means of a Voltaic battery, containing from one hundred to one hundred and fifty plates of four or six inches.

[This note is deserving the attention of those historians of science, before alluded to, who attributed the author's brilliant success in electro-chemical research to his supposed extraordinary means, the enormous Voltaic batteries of the Royal Institution.]

absorption of oxygen; and which occasioned the evolution of hydrogen from water, leaving pure mercury, and producing a solution of barytes.

Mixtures of lime, strontites, magnesia, and red oxide of mercury, treated in the same manner, gave similar amalgams, from which the alkaline earths were regenerated by the action of air or water, with like phenomena; but the quantities of metallic substances obtained were exceedingly minute; they appeared as mere superficial formations surrounding the point of the wire, nor did they increase after the first few minutes of electrization, even when the process was carried on for some hours.

These experiments were made previous to April, 1808, at which time the batteries were so much injured by constant use, as no longer to form an efficient combination. The inquiry was suspended for a short time: but in May I was enabled to resume it, by employing a new and much more powerful combination, constructed in the Laboratory of the Royal Institution, and consisting of five hundred pairs of double plates of six inches square.

When I attempted to obtain amalgams with this apparatus, the transmitting wires being of platina, of about $\frac{1}{40}$ of an inch in diameter; the heat generated was so great as to burn both the mercury and basis of the amalgam at the moment of its formation; and when by extending the surfaces of the conductors, this power of ignition was modified, yet still the amalgam was only produced in thin films, and I could not obtain globules sufficiently large to submit to distillation. When the transmitting wires were of iron of the same thickness, the iron acquired the temperature of ignition, and combined with the bases of the earths in preference to the mercury, and metallic alloys of a dark grey colour were obtained, which acted on water with the evolution of hydrogen, and were converted into oxide of iron, and alkaline earths.

Whilst I was engaged in these experiments, in the beginning of June, I received a letter from Professor Berzelius of Stockholm, in which he informed me that in conjunction with Dr. Pontin, he had succeeded in decomposing barytes and lime, by negatively electrifying mercury in contact with them, and that in this way he had obtained amalgams of the metals of these earths.

I immediately repeated these operations with perfect success; a globule of mercury, electrified by the power of the battery of 500, weakly charged, was made to act upon a surface of slightly moistened barytes, fixed upon a plate of platina. The mercury gradually became less fluid, and after a few minutes was found covered with a white film of barytes; and when the amalgam was thrown into water, hydrogen was disengaged, the mercury remained free, and a solution of barytes was formed.

The result with lime, as these gentlemen had stated, was precisely analogous.

That the same happy methods must succeed with strontites and magnesia, it was not easy to doubt, and I quickly tried the experiment.

From strontites I obtained a very rapid result; but from magnesia, in the first trials, no amalgam could be procured. By continuing the process, however, for a longer time, and keeping the earth continually moist, at last a combination of the basis with mercury was obtained, which slowly produced magnesia by absorption of oxygen from air, or by the action of water.

All these amalgams I found might be preserved for a considerable period under naphtha. In a length of time, however, they became covered with a white crust under this fluid. When exposed to air, a very few minutes only were required for the oxigenation of the bases of the earths. In water the amalgam of barytes was most rapidly decomposed: that of strontites and that of lime next in order: but the amalgam from magnesia, as might be expected from the weak affinity of the earth for water, very slowly changed; when a little sulphuric acid was added to the water, however, the evolution of hydrogen, and the production and solution of magnesia were exceedingly rapid, and the mercury soon remained free.

I was inclined to believe that one reason why magnesia was less easy to metallize than the other alkaline earths, was its insolubility in water, which would prevent it from being presented in the nascent state, detached from its solution at the negative surface. On this idea I tried the experiment, using moistened sulphate of magnesia, instead of the pure earth; and I found that the amalgam was much sooner obtained. Here the magnesia was attracted from the sulphuric acid, and probably deoxigenated and combined with the quicksilver at the same instant.

The amalgams of the other bases of the alkaline earths, could, I found, be obtained in the same manner from their saline compounds.

I tried in this way very successfully, muriate and sulphate of lime, the muriate of strontites, and of barytes, and nitrate of barytes. The earths separated at the deoxigenating surface, there seemed instantly to undergo decomposition, and seized upon by the mercury, were in some measure defended from the action of air, and from the contact of water, and preserved by their strong attraction for this metal.

III. *Attempts to procure the Metals of the Alkaline Earths; and on their Properties.*

To procure quantities of amalgams sufficient for distillation, I combined the methods I had before employed, with those of MM. Berzelius and Pontin.

The earths were slightly moistened, and mixed with one-third of red oxide of mercury, the mixture was placed on a plate of platina, a cavity was made in the upper part of it to receive a globule of mercury, of from 50 to 60 grains in weight, the whole was covered by a film of naphtha, and the plate was made positive, and the mercury negative, by a proper communication with the battery of 500.

The amalgams obtained in this way, were distilled in tubes of plate plass, or in some cases in tubes of common glass. These tubes were bent in the middle, and the extremities were enlarged, and rendered globular by blowing, so as to serve the purposes of a retort and receiver.

The tube, after the amalgam had been introduced, was filled with naphtha, which was afterwards expelled by boiling, through a small orifice in the end corresponding to the receiver, which was hermetically sealed when the tube contained nothing but the vapour of naphtha, and the amalgam.

I found immediately that the mercury rose pure by distillation from the amalgam, and it was very easy to separate a part of it; but to obtain a complete decomposition was very difficult.

For this nearly a red heat was required, and at a red heat the bases of the earths instantly acted upon the glass, and became oxigenated. When the tube was large in proportion to the quantity of amalgam, the vapour of the naphtha furnished oxygen sufficient to destroy part of the bases: and when a small tube was employed, it was difficult to heat the part used as a retort sufficient to drive off the whole of the mercury from the basis, without raising too highly the temperature of the part serving for the receiver, so as to burst the tube.*

In consequence of these difficulties, in a multitude of trials, I obtained only a very few successful results, and in no case could I be absolutely certain that there was not a minute portion of mercury still in combination with the metals of the earths.

In the best result that I obtained from the distillation of the amalgam of barytes, the residuum appeared as a white metal of the colour of silver. It was fixed at all common temperatures, but became fluid at a heat below redness, and did not rise in vapour when heated to redness, in a tube of plate glass, but acted violently upon the glass, producing a black mass, which seemed to contain barytes, and a fixed alkaline basis, in the first degree of oxigenation.†

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* When the quantity of the amalgam was about fifty or sixty grains, I found that the tube could not be conveniently less than one-sixth of an inch in diameter, and of the capacity of about half a cubic inch.

† From this fact, compared with other facts that have been stated, p. 105, it may be conjectured, that the basis of barytes has a higher affinity for oxygen than sodium; and hence, probably the bases of the earths will be more powerful instruments for detecting oxygen, than the bases of the alkalies.

I have tried a number of experiments on the action of potassium on bodies supposed simple, and on the undecompounded acids. From the affinity of the metal for oxygen, and on the acid for the substance

When exposed to air, it rapidly tarnished, and fell into a white powder, which was barytes. When this process was conducted in a small portion of air, the

formed, I had entertained the greatest hopes of success. It would be inconsistent with the object of this paper to enter into a full detail of the methods of operation; I hope to be able to state them fully to the Society at a future time, when they shall be elucidated by further researches; I shall now merely mention the general results, to shew that I have not been tardy in employing the means which were in my power, towards effecting these important objects.

When potassium was heated in muriatic acid gas, as dry as it could be obtained by common chemical means, there was a violent chemical action with ignition; and when the potassium was in sufficient quantity, the muriatic acid gas wholly disappeared, and from one-third to one-fourth of its volume of hydrogen was evolved, and muriate of potash was formed.

On fluoric acid gas, which had been in contact with glass, the potassium produced a similar effect; but the quality of hydrogen generated was only one-sixth or one-seventh of the volume of gas, and a white mass was formed, which principally consisted of fluate of potash and silex, but which emitted fumes of fluoric acid when exposed to air.

When boracic acid, prepared in the usual manner, that had been ignited, was heated in a gold tube with potassium, a very minute quantity of gas only was liberated, which was hydrogen, mixed with nitrogen, (the last probably from the common air in the tube); borate of potash was formed, and a black substance, which became white by exposure of air.

In all these instances there is great reason to believe that the hydrogen was produced from the water adhering to the acids; and the different proportions of it in the different cases, are a strong proof of this opinion. Admitting this idea, it seems that muriatic acid gas must contain at least one-eighth or one-tenth of its weight of water; and that the water oxigenates in the experiment a quantity of potassium, sufficient to absorb the whole of the acid.

In the cases of fluoric and boracic acids, there is probably a decomposition of these bodies; the black substance produced from the boracic acid is similar to that which I had obtained from it by electricity. The quantities that I have operated upon, have been as yet too small to enable me to separate and examine the products, and till this is done, no ultimate conclusion can be drawn.

The action of potassium upon muriatic acid gas, indicates a much larger quantity of water in this substance, than the action of electricity oxygen was found absorbed, and the nitrogen unaltered; when a portion of it was introduced into water, it acted upon it with great violence and sunk to the bottom, producing in it barytes; and hydrogen was generated. The quantities in which I obtained it were too minute for me to be able to examine correctly, either its physical or chemical properties. It sunk rapidly in the water, and even in sulphuric acid, though surrounded by globules of hydrogen, equal to two or three times its volume; from which it seems probable, that it cannot be less than four or five times as heavy as water. It flattened by pressure, but required a considerable force for this effect.

The metal from strontites sunk in sulphuric acid, and exhibited the same characters as that from barytes, except in producing strontites by oxidation.

The metal from lime, I have never been able to examine exposed to air or under naphtha. In the case in which I was able to distil the quicksilver from it to the greatest extent, the tube unfortunately broke, whilst warm, and at the moment that the air entered, the metal, which had the colour and lustre of silver, instantly took fire, and burnt with an intense white light into quicklime.

The metal from magnesia seemed to act upon the glass, even before the whole of the quicksilver was distilled from it. In an experiment in which I stopped

in Dr. Henry's elaborate experiments; but in the one instance the acid enters into a solid salt, and in the other it remains aëriform; and the difficulty of decomposition by electricity, must increase in proportion as the quantity of water diminishes, so that at the apparent maximum of electrical effect, there is no reason to suppose the gas free from water.

Those persons who have supposed hydrogen to be the basis of muriatic acid may, perhaps, give another solution of the phenomena, and consider the experiment I have detailed as a proof of this opinion.

the process before the mercury was entirely driven off, it appeared as a solid, having the same whiteness and lustre as the other metals of the earths. It sunk rapidly in water, though surrounded by globules of gas, producing magnesia, and quickly changed in air, becoming covered with a white crust, and falling into a fine powder, which proved to be magnesia.

In several cases in which amalgams of the metals of the earths, containing only a small quantity of mercury were obtained, I exposed them to air on a delicate balance, and always found that during the conversion of metal into earth, there was a considerable increase of weight.

I endeavoured to ascertain the proportions of oxygen, and bases, in barytes and strontites, by heating amalgams of them in tubes filled with oxygen, but without success. I satisfied myself, however, that when the metals of the earths were burned in a small quantity of air they absorbed oxygen, gained weight in the process, and were in the highly caustic or unslacked state; for they produced strong heat by the contact of water, and did not effervesce during their solution in acids.

The evidence for the composition of the alkaline earths is then of the same kind as that for the composition of the common metallic oxides; and the principles of their decomposition are precisely similar, the inflammable matters in all cases separating at the negative surface in the Voltaic circuit, and the oxygen at the positive surface.

These new substances will demand names; and on the same principles as I have named the bases of the fixed alkalies, potassium and sodium, I shall venture to denominate the metals from the alkaline earths barium, strontium, calcium, and magnium; the last of these words is undoubtedly objectionable, but magnesium* has been already applied to metallic manganese, and would consequently have been an equivocal term.

IV. *Inquiries relative to the Decomposition of Alumine, Silex, Zircone, and Glucine.*

I tried the methods of electrization and combination with quicksilver, and the common metals, by which I had succeeded in decomposing the alkaline earths, on alumine and silex; but without gaining distinct evidences of their having undergone any change in the processes.

Obliged to seek for other means of acting upon them, it was necessary to consider minutely their relations to other bodies, and to search for analogies by which the principles of research might be guided.

Alumine very slowly finds its point of rest at the negative pole, in the electrical circuit; but silex, even when diffused in its gelatinous state through water, rests indifferently at the negative or positive poles.

From this indifference to positive and negative electrical attractions, following the general order of facts, it might be inferred, that if these bodies be compounds, the electrical energies of their elements are nearly in equilibrium; and that their state is either analogous to that of insoluble neutral salts, or of oxides nearly saturated with oxygen.

The combinations of silex and alumine, with acids and alkalies, as well as their electrical powers, were not inconsistent with either of these ideas; for in some respects they resemble in physical characters, fluate and phosphate of lime, as much as, in others, they approach to the oxides of zinc and tin.

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* Bergman, Opusc. tom. ii. p. 200. [This term he afterwards preferred, vide Vol. IV. Coll. Works, p. 258.]

On the idea that silex might be an insoluble neutro-saline compound, containing an unknown acid or earth, or both, and capable of being resolved into its secondary elements, in the same manner as sulphate of barytes, or fluate of lime, I made the following experiments.

Two gold cones,* connected by moistened amianthus, were filled with pure water, and placed in the electrical circuit, a small quantity of carefully prepared and well washed silex was introduced into the positive cone: the action was kept up from a battery of two hundred plates, for some hours till nearly half of the fluid in each cone was exhausted; the remainders were examined; the fluid in the cone containing the silex was strongly acid; that in the opposite cone was strongly alkaline; the two fluids were passed through bibulous paper, and mixed together, when a precipitate fell down, which proved to be silex.

On the first view of the subject, it appeared probable that this silex had been formed by the union of the acid and the alkaline matter in the two cones, and that the experiment demonstrated a decomposition and recomposition of silex; but before such a conclusion could be made, many points were to be determined.

It was possible that the acid might be nitric acid, produced as in other electrical experiments of a similar nature, and that this acid might have dissolved silex, which was precipitated by the alkaline matter at the other pole, which might be either potash used for dissolving the silex, which had adhered to it, notwithstanding the processes of lixiviation in acids, or ammonia produced in consequence of the presence of the atmosphere; or if potash was present, it was likewise possible that the silex might have been carried over in solution, with this alkali, from the positive to the negative surface.

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* The same as those described in p. 6. of this vol.

Minute experiments were instituted and completed in the same manner as those detailed in the Philosophical Transactions for 1807, p. 7,* which soon proved that there was no reason to suppose that the silex had been changed in these experiments.

The acid proved to be nitric acid, which under the electrical action seemed to have dissolved the silex; the alkali turned out to be principally fixed alkali; and that it was merely an accidental ingredient, and not a constituent of the silex, appeared from this circumstance, that when the same portion of silex was long electrified, by degrees it lost its power of affording the substance in question.†

This result having taken place, the same plan of operation was not pursued with respect to alumine, which resembles a saline compound less than silex, and the method which I now adopted of acting upon

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[* Page 7. of this volume.]

† If silex that has been carefully washed, after precipitation by muriatic acid from liquor silicum, be moistened, and acted on by mercury negatively electrified, the mercury soon contains a notable quantity of potassium. Well washed alumine that has been precipitated from alum by carbonate of soda, affords by the same treatment sodium and potassium, so that the powers of electro-chemical analysis are continually demonstrating the imperfection of the common chemical methods of separating bodies from each other. The purest boracic acid which can be obtained from borax by chemical decomposition, by electrical analysis is shewn to contain both soda, and the decomposing acid employed in the process; and hence the experiment on the action of the boracic acid and potassium, page 113, may possibly be explained without assuming its decomposition.

these bodies, was on the supposition of their being inflammable substances so highly saturated with oxygen as to possess little or no positive electricity.

Alumine and silex have both a strong affinity for potash and soda; now supposing them to be oxides, it was reasonable to conclude that the oxygen, both in the alkalies and the earths, must be passive as to this power, which must consequently be referred to their bases, and on this notion it was possible that it might be made to assist their decomposition by electricity.

After this reasoning, I fused a mixture of one part of silex, and six of potash in a platina crucible, and preserved the mixture fluid, and in ignition, over a fire of charcoal; the crucible was rendered positive from the battery of five hundred, and a rod of platina, rendered negative, was brought in contact with the alkaline menstruum. At the moment of contact there was a most intense light; when the rod was plunged into the liquid an effervescence took place, and globules which burnt with a brilliant flame rose to the surface, and swam upon it in a state of combustion. In a few minutes, when the mixture was cool, the platina bar was removed: after as much as possible of the alkali and silex had been detached from it by a knife, there remained brilliant metallic scales round it, which instantly became covered with a white crust in the air, and some of which inflamed spontaneously. The platina appeared much corroded, and of a darker tint than belongs to the pure metal. When it was plunged into water it strongly effervesced: the fluid that came from it was alkaline; when a few drops of muriatic acid were added to the solution, a white cloudiness occurred, which various trials demonstrated, depended upon the presence of silex.

A similar mixture of potash and alumine was experimented upon in the same manner, and the results were perfectly analogous; there adhered to the rod of platina a film of a metallic substance, which rapidly decomposed water, and afforded a solution which deposited alumine by the action of an acid.

I tried several forms of this experiment, with the hopes of being able to obtain a sufficient quantity of the metallic matter from the platina, so as to examine it in a separate state; but I was not successful. It was always in superficial scales, which oxidated, becoming white and alkaline, before it could be detached in the air; it instantly burnt when heated, and could not be fused under naphtha or oil.

I tried similar experiments with mixtures of soda and alumine, and soda and zircone, and used iron as the negatively electrified metal. In all these cases, during the whole process of electrization, abundance of globules, which swam in a state of inflammation on the fused mass, were produced. And in the mixture, when cooled, small laminæ of metal were found of the colour of lead, and less fusible than sodium, which adhered to the iron; they acted violently upon water, and produced soda and a white powder, but in quantities too small to be minutely examined.

I endeavoured to procure an alloy of potassium, and the bases of the earths, from mixtures of potash, silex, and alumine, fused by electricity, and acted on by the positive and negative surfaces in the same manner as pure potash, in experiments for the decomposition of that substance; but I obtained no good results. When the earths were in quantities equal to one-fourth or one-fifth of the alkali, they rendered it so highly non-conducting, that it was not easy to affect it by electricity, and when they were in very minute portions, the substance produced had the characters of pure potassium.

I heated small globules of potassium, in contact with silex and alumine, in tubes of plate glass filled with the vapour of naphtha: the potassium seemed to act at the same time upon the glass and the earths, and a grayish opaque mass, not possessed of metallic splendour was obtained, which effervesced in water, depositing white clouds. Here it was possible that the potash had been converted wholly or partly into protoxide, by its action upon the earths; but as no globule was obtained, and as the plate glass alone might have produced the effect, no decided inference of the decomposition of the earths can be drawn from the process.

I shall now mention the last trials that I made with respect to this object.

Potassium, amalgamated with about one-third of mercury, was electrified negatively under naphtha, in contact with silex very slightly moistened, by the power of five hundred; after an hour the result was examined. The potassium was made to decompose water, and the alkali formed neutralized by acetous acid; a white matter, having all the appearance of silex precipitated, but in quantity too small for accurate examination.

I tried the same method of action upon alumine and glucine, and obtained a cloudiness, more distinct than in the case of silex, by the action of an acid upon the solution obtained from the amalgam.

Zircone exposed in the same manner to the action of electricity, and the attraction of potassium, furnished still more satisfactory results, for a white and fine powder, soluble in sulphuric acid, and which was precipitated from sulphuric acid by ammonia, separated from the amalgam that had been obtained, by the action of water.

From the general tenor of these results, and the comparison between the different series of experiments, there seems very great reason to conclude that alumine, zircone, glucine, and silex are, like the alkaline earths, metallic oxides, for on no other supposition is it easy to explain the phenomena that have been detailed.

The evidences of decomposition and composition, are not, however, of the same strict nature as those that belong to the fixed alkalies and alkaline earths; for it is possible, that in the experiments in which the silex, alumine, and zircone appeared to separate during the oxidation of potassium and sodium, their bases might not actually have been in combination with them, but the earths themselves, in union with the metals of the alkalies, or in mere mechanical mixture. And out of an immense number of experiments which I made of the kind last detailed, a very few only gave distinct indications of the production of any earthy matter; and in cases when earthy matter did appear, the quantity was such as rendered it impossible to decide on the species.

Had I been so fortunate as to have obtained more certain evidences on this subject, and to have procured the metallic substances I was in search of, I should have proposed for them the names of silicium, alumium, zirconium, and glucium.

V. *On the Production of an Amalgam from Ammonia, and on its Nature and Properties.*

In the communication from Professor Berzelius and Dr. Pontin, which I have already referred to, a most curious and important experiment on the deoxidation and amalgamation of the compound basis of ammonia is mentioned, which these ingenious gentlemen regard as a strict proof of the idea I had formed of its being an oxide with a binary basis.

Mercury, negatively electrified in the Voltaic circuit, is placed in contact with solution of ammonia. Under this agency it gradually increases in volume, and when expanded to four or five times its former dimensions, becomes a soft solid.

And that this substance is composed of the deoxigenated compound basis of ammonia and mercury, they think is proved; 1. By the reproduction of quicksilver and ammonia, with the absorption of oxygen, when it is exposed to air; and secondly, by its forming ammonia in water, whilst hydrogen is evolved, and the quicksilver gradually becomes free.

An operation, in which hydrogen and nitrogen exhibit metallic properties, or in which a metallic substance is apparently composed from its elements, cannot fail to fix the attention of chemists: and the peculiar interest which it offered in its relations to the general theory of electro-chemical science, induced me to examine the circumstances connected with it minutely and extensively.

In repeating the process of the Swedish chemists, I found that to form an amalgam from fifty or sixty grains of mercury, in contact with saturated solution of ammonia, required a considerable time, and that this amalgam greatly changed even in the short period required for removing it from the solution.

I was however able, in this mode of operating, to witness all the results they have stated, and I soon found simple and more easy means of producing the effect, and circumstances under which it could be more distinctly analysed.

The experiments which I have detailed in the Bakerian Lecture for 1806, proved that the ammonia is disengaged from the ammoniacal salts, at the negative surface in the Voltaic circuit; and I concluded that under this agency, it may be acted on in what is called the nascent state, when it was reasonable to conclude it would be more readily deoxigenated and combined with quicksilver.

On this view of the subject, I made a cavity in a piece of muriate of ammonia; into this a globule of mercury, weighing about fifty grains, was introduced. The muriate was slightly moistened, so as to be rendered a conductor, and placed on a plate of platina, which was made positive in the circuit of the large battery. The quicksilver was made negative by means of a platina wire. The action of the quicksilver on the salt was immediate; a strong effervescence with much heat took place. The globule in a few minutes had enlarged to five times its former dimensions, and had the appearance of an amalgam of zinc; and metallic crystallizations shot from it, as a centre, round the body of the salt. They had an arborescent appearance, often became coloured at their points of contact with the muriate; and when the connection was broken, rapidly disappeared, emitting ammoniacal fumes, and reproducing quicksilver.

When a piece of moistened carbonate of ammonia* was used, the appearances were the same, and the amalgam was formed with equal rapidity. In this process of deoxidation, when the battery was in powerful action, a black matter formed in the cavity, which there is

* [Sesquicarbonate.]

every reason to believe was carbonaceous matter from the decomposition of the carbonic acid of the carbonate.*

The strong attraction of potassium, sodium, and the metals of the alkaline earths for oxygen, induced me to examine whether their deoxidating powers could not be made to produce the effect of the amalgamation of ammonia, independently of the agency of electricity; and the result was very satisfactory.

When mercury, united to a small quantity of potassium, sodium, barium, or calcium, was made to act upon moistened muriate of ammonia, the amalgam rapidly increased to six or seven times its volume, and the compound seemed to contain much more ammoniacal basis than that procured by electrical powers.

As in these cases, however, a portion of the metal used for the deoxidation always remained in union in the compound; in describing the properties of the amalgam from ammonia, I shall speak only of that procured by electrical means.

The amalgam from ammonia, when formed at the temperature of 70° or 80°, is a soft solid, of the consistence of butter; at the freezing temperature it becomes firmer, and a crystallized mass, in which small facets appear, but having no perfectly defined form.† Its specific gravity is below 3, water being one.

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* The black matter which separates at the negative surface in the electrical experiments on the decomposition of potash or soda, and which some experimenters have found it difficult to account for, is I find carbonaceous, and dependent upon the presence of carbonic acid in the alkali.

† From the facet I suspect the form to be cubical. The amalgam of potassium crystallizes in cubes as beautiful, and in some cases as large, as those of bismuth.

When exposed to air it soon becomes covered with a white crust, which proves to be carbonate of ammonia.

When thrown into water it produces a quantity of hydrogen, equal to about half its bulk, and in consequence of this action the water becomes a weak solution of ammonia.

When it is confined in a given portion of air, the air enlarges considerably in volume, and the pure quicksilver re-appears. Ammoniacal gas, equal to one and a half or one and three-fifths of the volume of the amalgam is found to be produced, and a quantity of oxygen equal to one-seventh, or one-eighth of the ammonia disappears.*

When thrown into muriatic acid gas, it instantly becomes coated with muriate of ammonia, and a small quantity of hydrogen is disengaged.

In sulphuric acid it becomes coated with sulphate of ammonia and sulphur.

I attempted by a variety of modes to preserve this amalgam. I had hoped by submitting it to distillation out of the contact of air, or water, or bodies which could furnish oxygen, to be able to obtain the deoxigenated substance which had been united to the quicksilver in a pure form; but all the circumstances of the experiment opposed themselves to such a result.

It is well known to persons accustomed to barometrical experiments, that mercury, after being once moistened, retains water with great perseverance, and can only be freed from it by boiling; and in the cases of the decomposition of ammonia, when a soft amalgam had been kept continually moist, both internally and externally for some time, it could not be expected that all the water adhering to it should be easily removed.

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* This experiment confirms the opinions I have stated concerning the quantity of oxygen in ammonia; but as water is present, as will be immediately shewn, the data for proportions are not perfectly correct.

I wiped the amalgam as carefully as possible with bibulous paper; but even in this process a considerable portion of the ammonia was regenerated; I attempted to free it from moisture by passing it through fine linen, but a complete decomposition was effected, and nothing was obtained but pure quicksilver.

The whole quantity of the basis of ammonia combined in sixty grains of quicksilver, as is evident from the statements that have been made, does not exceed $\frac{1}{200}$ part of a grain, and to supply oxygen to this scarcely $\frac{1}{1000}$ part of a grain of water would be required, which is a quantity hardly appreciable, and which merely breathing upon the amalgam would be almost sufficient to communicate.

Hence, when an amalgam, which had been wiped by means of bibulous paper, was introduced into naphtha, it decomposed almost as rapidly as in the air, producing ammonia and hydrogen.

In oils it evolved hydrogen, and generated ammoniacal soap; and when it was introduced into a glass tube, closed by a cork, gas was rapidly formed, and the mercury remained free; and this gas, when examined, was found to consist of from about two-thirds to three-fourths ammonia, and the remainder hydrogen.*

That more moisture sometimes existed attached to the amalgam, when wiped as dry as possible by bibulous paper, than was sufficient for the effect of decomposition, I soon found by an experiment of distillation.

About a quarter of a cubic inch of an amalgam nearly solid was wiped very dry, and introduced into a small tube: in this tube it was heated till the gaseous matter had expelled the quicksilver; the tube was then closed, and suffered to cool, when moisture, which proved to be a saturated solution of ammonia, had precipitated upon it.

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* In the experiment of the action of the amalgam upon air, the oxygen is probably absorbed by nascent hydrogen, and reproduces water, which is dissolved by the ammonia.

I have mentioned that the amalgams obtained from ammonia, by means of the metals of the fixed alkalies or alkaline earths, seemed to contain much more ammoniacal basis in combination than those procured by electricity: and when they are combined with the metals of the fixed alkalies or of the earths in any considerable quantities, they are much more permanent.

Triple compounds of this kind, when carefully wiped, scarcely produce any ammonia under naphtha, or oil, and may be preserved for a considerable time in closed glass tubes, a little hydrogen being the only product evolved from them.

I heated a triple amalgam obtained from ammonia by potassium, and which had been wiped by bibulous paper in a dry plate-glass tube over mercury; a considerable elevation of temperature was required before any gaseous matter was emitted, but the heat was raised till gas was rapidly formed, and the whole of the amalgam expelled from the tube: in cooling, the mercury rose very quickly in it, so that a great part of the gaseous matter had been either mercury or water, in vapour, or something which the mercury had absorbed in cooling. The small quantity which was permanent, did not equal one-half the volume of the amalgam.

On the idea that this gas might be a compound of hydrogen and nitrogen in the state of deoxigenation, I mixed a small quantity of oxygen gas with it, but no change of volume took place; I then exposed it to naphtha, when one-half of it was absorbed, which by the effect the naphtha produced upon turmeric must have been ammonia; the remaining gas analyzed was found to consist of the oxygen that had been introduced, and of 1 hydrogen and nitrogen to each other in the proportion of nearly four to one.

At first I was perplexed by this result, which seemed to prove the production of ammonia, independent of the presence of any substance which could furnish oxygen to it, and to shew that its amalgamation was merely owing to its being freed from water, and combined with hydrogen: but a satisfactory solution of the difficulty soon offered itself. Exposing the triple amalgam procured from ammonia by potassium to a concentrated solution of ammonia, I found that it had very little action upon it, and introducing the amalgam moistened by it into a glass tube, it had nearly the same permanency as the amalgam which had been wiped before it was introduced, a little hydrogen only being evolved; but on heating the tube gaseous matter was rapidly generated, which proved to consist of two-thirds ammonia, and one-third hydrogen.

In the instance in which the amalgam had been wiped, a small quantity of solution of ammonia, and perhaps of potash, must have adhered to it; and though the amalgam does not act upon this powerfully at common temperatures, yet when the water is raised in vapour, it tends to oxigenate both the basis of ammonia, and potassium, and hence hydrogen is evolved, and volatile alkali produced.

I distilled an amalgam procured by potassium from ammonia, in a tube filled with the vapour of naphtha, and hermetically sealed, in the same manner as in the experiments for obtaining the metals of the earths, but in this case I procured ammonia, hydrogen, and nitrogen only, and pure mercury; and the residuum was potassium, which acted powerfully on the glass tube.

In another experiment of the same kind, I kept one part of the tube cool by ice, at the time the other part was strongly heated, but nothing condensable except mercury was produced, and the elastic products were the same as in the former instance.

I endeavoured to procure an amalgam from ammonia, to which no moisture could be supposed to adhere, by heating an amalgam of potassium in ammoniacal gas. The amalgam became covered with a film of potash, but it did not enlarge in its dimensions, and a considerable quantity of non-absorbable gas, which was found to consist of five parts of hydrogen, and one of nitrogen, was produced. The amalgam after this operation did not emit ammonia by exposure to air; hence it seems probable, that for the deoxigenation of ammonia, and the combination of its basis with mercury, the alkali must be in the nascent state, or at least in that condensed form in which it exists in ammoniacal salts, or solutions.*

VI. *Some Considerations of general Theory, connected with the Metallization of the Alkalies and the Earths.*

The more the properties of the amalgam obtained from ammonia are considered, the more extraordinary do they appear.

Mercury by combination with about $\frac{1}{12000}$ part of its weight of new matter, is rendered a solid, yet has its specific gravity diminished from 13·5 to less than 3, and it retains all its metallic characters; its colour, lustre, opacity, and conducting powers remaining unimpaired.

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* [The deoxigenation above alluded to is of doubtful reality,—as the author shews farther on.]

It is scarcely possible to conceive that a substance which forms with mercury so perfect an amalgam, should not be metallic in its own nature;* and on this idea to assist the discussion concerning it, it may be conveniently termed ammonium.

But on what do the metallic properties of ammonium depend?

Are hydrogen and nitrogen both metals in the aëriform state, at the usual temperatures of the atmosphere, bodies of the same character, as zinc and quicksilver would be in the heat of ignition?

Or are these gases, in their common form, oxides, which become metallized by deoxidation?

Or are they simple bodies not metallic in their own nature, but capable of composing a metal in their deoxigenated, and an alkali in their oxigenated state?

These problems, the second of which was stated by Mr. Cavendish to me, and the last of which belongs to M. Berzelius, offer most important objects of investigation.

I have made some experiments in relation to them, but as yet unsuccessfully. I have heated the amalgam of potassium, in contact with both hydrogen and nitrogen, but without attaining their metallization; but this fact cannot be considered as decisively for or against any one of these conjectures.

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* The nature of the compounds of sulphur and phosphorus with mercury, favours this opinion; these inflammable bodies by combination, impair its metallic properties; cinnabar is a non-conductor, and it would seem from Pelletier's experiments, Ann. de Chimie, vol. xiii. p. 125, that the phosphuret of mercury is not metallic in its characters; charcoal is a conductor, and in plumbago carbon approaches very near to a metal in its characters, so that the metallic nature of steel does not militate against the reasoning in the text. The only facts which I am acquainted with, that do militate against it, are the metallic characters of some of the sulphurets and phosphurets of the imperfect metals.

I mentioned in the Bakerian Lecture for 1807, that a modification of a phlogistic chemical theory might be defended on the idea that the metals and inflammable solids, usually called simple, were compounds of the same matter as that existing in hydrogen, with peculiar unknown bases, and that the oxides, alkalies, and acids were compounds of the same bases with water, and that the phenomena presented by the metals of the fixed alkalies might be explained on this hypothesis.

The same mode of reasoning may be applied to the facts of the metallization of the earths and ammonia, and perhaps with rather stronger evidences in its favour, but still it will be less distinct and simple, than the usually received theory of oxigenation, which I have applied to them.

The general facts of the combustion, and of the action of these new combustible substances upon water, are certainly most easily explained on the hypothesis of Lavoisier; and the only good arguments in favour of a common principle of inflammability, flow from some of the novel analogies in electro-chemical science.

Assuming the existence of hydrogen in the amalgam of ammonium, its presence in one metallic compound evidently leads to the suspicion of its combination in others. And in the electrical powers of the different species of matter, there are circumstances which extend the idea to combustible substances in general. Oxygen is the only body which can be supposed to be elementary, attracted by the positive surface in the electrical circuit, and all compound bodies, the nature of which is known, that are attracted by this surface, contain a considerable proportion of oxygen. Hydrogen is the only matter attracted by the negative surface, which can be considered as acting the opposite part to oxygen; may not then the different inflammable bodies, supposed to be simple, contain this as a common element?

Should future experiments prove the truth of this hypothesis, still the alkalies, the earths, and the metallic oxides will belong to the same class of bodies. From platina to potassium there is a regular order of gradation as to their physical and chemical properties, and this would probably extend to ammonium, could it be obtained in the fixed form. Platina and gold in specific gravity, degree of oxidability, and other qualities, differ more from arsenic, iron, and tin, than these last do from barium and strontium. The phenomena of combustion of all the oxidable metals are precisely analogous. In the same manner as arsenic forms an acid by burning in air, potassium forms an alkali and calcium an earth; in a manner similar to that in which osmium forms a volatile and acrid substance by the absorption of oxygen, does the amalgam of ammonium produce volatile alkali; and if we suppose that ammonia is metallized, by being combined with hydrogen, and freed from water, the same reasoning will likewise apply to the other metals, with this difference, that the adherence of their phlogiston or hydrogen, would be exactly in the inverse ratio of their attraction for oxygen. In platina* it would be combined with the greatest energy; in ammonium with the least; and if it be separable from any of the metals without the aid of a new combination, we may expect that this result will be afforded by the most volatile and oxidable, such as arsenic, or the metals of the fixed alkalies, submitted to intense heat, under electrical polarities, and having the pressure of the atmosphere removed.

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* The common metallic oxides are lighter than their bases, but potash and soda are heavier; this fact may be explained on either theory; the density of a compound will be proportional to the attraction of its parts. Platina, having a weak affinity for oxygen, cannot be supposed to condense it in the same degree as potassium does; or if platina and potassium be both compounds of hydrogen, the hydrogen must be attracted in platina, with an energy infinitely greater than in potassium. Sul-

Whatever new lights new discoveries may throw upon this subject, still the facts that have been advanced shew that a step nearer at least has been attained towards the true knowledge of the nature of the alkalies and the earths.*

phuric acid is lighter than sulphur; but phosphoric acid (where there is a stronger affinity) is heavier than phosphorus. The oxide of tin (wood tin) is very little inferior to tin in specific gravity. In this instance the metallic base is comparatively light, and the attraction for oxygen strong; and in a case when the metal is much lighter and the attraction for oxygen stronger, it might be expected *à priori* that the oxide would be heavier than the base.

* Since the facts in this Paper were communicated to the Royal Society, I have seen an account of some very curious experiments of MM. Gay Lussac and Thenard, (in No. 148 of the Moniteur, for 1808, which I have just received,) from one of which they have concluded, "that potassium may be a compound of hydrogen and potash."

These gentlemen are said to have heated potassium in ammonia, and found that the ammonia was absorbed, and that hydrogen gas equal to two-thirds of its volume appeared, and that the potassium by this process had become of a grayish-green colour. By heating this grayish-green substance considerably, two-fifths of the ammonia were again emitted, with a quantity of hydrogen and nitrogen corresponding to one-fifth more, and by adding water to the mixture, and heating it very strongly again, they obtained the remainder of the ammonia, and nothing but potash was left.

In these complex processes, the phenomena may be as easily explained on the idea of potassium being a simple, as that of its being a compound substance; nor when the facts that have been stated in this paper and those about to be stated, are considered, can the view of these distinguished chemists, as detailed in the notice referred to, be at all admitted.

Something has been separated from them which adds to their weight; and whether it be considered as oxygen, or as water, the inflammable body is less compounded,

Potash, as I have found by numerous experiments, has no affinity for ammonia, for it does not absorb it when heated in it; it is not therefore (allowing their theory) possible to conceive that a substance having no attraction for potash, should repel from it a substance which is intimately combined with it, and which can be separated in no other way.

A part of the hydrogen evolved in their experiment, may be furnished by water contained in the ammonia; but it is scarcely possible that the whole of it can be derived from this source, for on such an idea the ammonia must contain more than half its weight of water. There is however no evidence that the whole of the hydrogen may not be furnished by the decomposition of the volatile alkali itself. Potassium in its first degree of oxigenation, may have an affinity for nitrogen, or potassium may expel a portion of hydrogen at the moment of its combination with ammonium; and as the whole of the ammonia cannot be regenerated without the presence of water, hydrogen and a little oxygen may be furnished to the remaining elements of the ammonia, from the water, and oxygen to the potassium.

Even before the conclusion was formed, that a metallic substance is decomposed in this experiment, it should have been proved that the nitrogen had not been altered.

That mere potash, combined with hydrogen, cannot form potassium, is I think shown by an experiment which I tried, in consequence of the important fact lately ascertained by MM. Gay Lussac and Thenard, of the deoxidation of potash by iron.

An ounce of potash was kept in ignition for some time in an iron tube, ground into a gun barrel in which one ounce and a half of iron turnings were ignited to whiteness; a communication was opened, by withdrawing a wire which closed the tube containing the potash, between that alkali and the metal.

As the potash came in contact with the iron, gaseous matter was developed, which was received in a proper apparatus, and though some of it was lost by passing through the potash into the atmosphere, yet nearly half a cubic foot was preserved, which proved to be hydrogen. In the tube were found two products, one in the quantity of a few grains, containing potassium, combined with a small quantity of iron, and which had sublimed in the operation, and the other a fixed white metallic substance which consisted of an alloy of iron and potassium.

The first of these substances burnt when thrown upon water; and in its other characters resembled pure potassium, except that its specific than the uninflammable substance resulting from its combustion.

Other hypotheses might be formed upon the new

gravity was greater, its colour less brilliant, and when it tarnished in the atmosphere, it became of a much deeper colour than pure potassium.

Now potash that has been ignited, is the purest form known of this alkali; but on MM. Gay Lussac's and Thenard's theory, this potash must contain water, not only sufficient to furnish hydrogen to metallize the alkali, but likewise the quantity disengaged: dry potash then, as it is procured in our experiments, must on this theory be a compound, containing a considerable quantity of matter which can furnish hydrogen; and what would be its form or properties if deprived of this matter we are wholly unable to judge, which brings this question to the general question discussed in the text.

Potassium I find may be produced readily from dry ignited potash in electrical experiments; and the result of the combustion of potassium in oxygen gas is an alkali, so dry that it produces violent heat, and ebullition when water is added to it.

In MM. Gay Lussac's and Thenard's experiment on the action of potassium on ammonia, the hydrogen disengaged in the first process, and that existing in the ammonia disengaged in the second process, exactly equals the whole quantity contained in the ammonia. But there is no proof of any hydrogen being disengaged from the potassium, for the ammonia lost is not generated, nor potash formed, but by the addition of a substance, consisting of oxygen and hydrogen: and as the three bodies concerned in this experiment are potassium, ammonia, and water, the result ought to be potash, ammonia, and a quantity of hydrogen, equal to that evolved by the mere action of water on potassium, which is said to be the case.

Even if there were no other proofs, the chemical properties of potassium are so wholly unlike those that might be expected from a compound of potash and hydrogen, that they are almost sufficient to decide the question. Potassium acts upon water with much more energy than potash, and produces much more heat in it; and yet if a compound of hydrogen, the affinity of potash for water must be diminished by its affinity for hydrogen, to say nothing of the quantity of heat, which ought (on the common theory of capacity for heat) to be carried off by this light inflammable gas.

Potassium burns in carbonic acid, and precipitates charcoal from it; whereas hydrogen electrized with carbonic acid, converts it into gaseous oxide of carbon.

electro-chemical facts, in which still fewer elements than those allowed in the antiphlogistic or phlogistic theory might be maintained. Certain electrical states always coincide with certain chemical states of bodies. Thus acids are uniformly negative, alkalies positive, and inflammable substances highly positive; and as I have found, acid matters when positively electrified, and alkaline matters when negatively electrified, seemed to lose all their peculiar properties and powers of combination. In these instances the chemical qualities are shewn to depend upon the electrical powers; and it is not impossible that matter of the same kind, possessed of different electrical powers, may exhibit different chemical forms.*

Potash has a very slight attraction for phosphorus; but potassium has a very strong affinity for it, so as to separate it from hydrogen, and according to MM. Gay Lussac and Thenard, with the phenomena of inflammation. Potash has no affinity for arsenic, yet from the experiments of these gentlemen, it appears that potassium separates arsenic from arseniuretted hydrogen; and hydrogen, which is supposed by them to exist in both compounds, can have no affinity for hydrogen, nor can hydrogen in one form, be supposed capable of separating arsenic from hydrogen in another form.

Could not the experiment of MM. Gay Lussac and Thenard be explained, except on the supposition of the hydrogen being derived from the potassium, it would be a distinct fact in favour of the revival of the theory of phlogiston. It would not prove, however, that potassium is composed of hydrogen and potash, but that it is composed of hydrogen and an unknown basis; and that potash is this basis united to water.

* Phil. Trans. 1807, part 1. p. 23. [P. 23 of this Vol.] The amalgam obtained from ammonia offers difficulties to both the phlogistic and antiphlogistic hypotheses. If we assume the phlogistic hypothesis, then we must assume that nitrogen, by combining with one-fourth of its weight of hydrogen can form an alkali, and by combining with one-twelfth more, can become metallic. If we reason on the antiphlogistic hypothesis, we must assert, that though nitrogen has a weaker affinity for oxygen than hydrogen, yet a compound of hydrogen and nitrogen is capable of decomposing water.

The first assumption is however by far the most contradictory to the order of common chemical facts; the last, though it cannot be wholly

I venture to hint at these notions; but I do not attach much importance to them; the age of chemistry is not yet sufficiently mature for such discussions; the more subtile powers of matter are but just beginning to be considered; and all general views concerning them, must as yet rest upon feeble and imperfect foundations.

Whatever be the fate of the speculative part of the inquiry, the facts however will, I hope, admit of many applications, and explain some phenomena in nature.

The metals of the earths cannot exist at the surface of the globe; but it is very possible that they may form a part of the interior; and such an assumption would

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removed, is yet lessened by analogies. Thus alloys in general, and inflammable compounds, are more oxidable than the simple substances that compose them. Sulphuret of iron at common temperatures decomposes water with facility, whereas sulphur under the same circumstances, has no action on water, and iron a very small one. The compound of phosphorus and hydrogen is more inflammable than either of its constituents.

Should a new theory of the dependence of the chemical forms of matter upon electrical powers be established, the facts belonging to ammonium would admit of a more easy solution. Ammonium might be supposed to be a simple body, which by combining with different quantities of water, and in different states of electricity, formed nitrogen, ammonia, atmospherical air, nitrous oxide, nitrous gas, and nitric acid.

Water, on this idea, must be supposed a constituent part of all the different gases; but its electricities in oxygen and hydrogen would probably be the very reverse of what they have been supposed by M. Ritter, and some ingenious English inquirers.

Water positively electrified would be hydrogen, water negatively electrified oxygen; and as in the physical experiments of temperature, ice, added to certain quantities of steam by an equilibrium of heat produces water, so in the chemical experiment of the generation of water the positive and negative electricity of oxygen and hydrogen in certain proportions would annihilate each other, and water alone be the result. At all events ammonium, whether simple or compound, must be considered as owing its attraction for oxygen to its highly positive electrical state, which is shewn by its powerful determination to the negative surface in the Voltaic circuit.

offer a theory for the phenomena of volcanoes, the formation of lavas, and the excitement and effects of subterraneous heat,* and would probably lead to a general hypothesis in geology.

The luminous appearance of those meteors connected with the fall of stones, is one of the extraordinary circumstances of these wonderful phenomena. This effect may be accounted for, by supposing that the substances which fall, come into our atmosphere in a metallic state; and that the earths they principally consist of are a result of combustion ; but this idea has not the slightest connexion with their origin or causes.

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* Let it be assumed that the metals of the earths and alkalies, in alloy with common metals, exist in large quantities beneath the surface, then their accidental exposure to the action of air and water, must produce the effect of subterranean fire, and a produce of earthy and stony matter analogous to lavas. [This simple and brilliant hypothesis, the author ultimately relinquished, as will appear in the sequel ; he relinquished it, after diligent inquiry into the phenomena of active volcanoes, in consequence of not finding the facts in accordance with it. In this instance, as in every other, he set no value on hypothesis excepting as a guide to research.]

IV.

AN ACCOUNT OF SOME NEW ANALYTICAL RESEARCHES ON THE NATURE OF CERTAIN BODIES, PARTICULARLY THE ALKALIES, PHOSPHORUS, SULPHUR, CARBONACEOUS MATTER, AND THE ACIDS HITHERTO UNDECOMPOSED; WITH SOME GENERAL OBSERVATIONS ON CHEMICAL THEORY.*

I. *Introduction.*

In the following pages, I shall do myself the honour of laying before the Royal Society, an account of the results of the different experiments, made with the hopes of extending our knowledge of the principles of bodies by the new powers and methods arising from the applications of electricity to chemistry, some of which have been long in progress, and others of which have been instituted since their last session.

The objects which have principally occupied my attention, are the elementary matter of ammonia, the nature of phosphorus, sulphur, charcoal, and the diamond, and the constituents of the boracic, fluoric, and muriatic acids.

Amongst the numerous processes of decomposition, which I have attempted, many have been successful; and from those which have failed, some new phenomena have usually resulted which may possibly serve as guides in future inquiries. On this account, I shall keep back no part of the investigation, and I shall trust to the candour of the Society for an excuse for its imperfection.

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* [From Phil. Trans. for 1809. Read before the Royal Society, Dec. 15, 1808.]

The more approaches are made in chemical inquiries towards the refined analysis of bodies, the greater are the obstacles which present themselves, and the less perfect the results.

All the difficulties which occur in analysing a body, are direct proofs of the energy of attraction of its constituent parts. In the play of affinities with respect to secondary compounds even, it rarely occurs that any perfectly pure or unmixed substance is obtained; and the principle applies still more strongly to primary combinations.

The first methods of experimenting on new objects likewise are necessarily imperfect; novel instruments are demanded, the use of which is only gradually acquired, and a number of experiments of the same kind must be made, before one is obtained from which correct data for conclusions can be drawn.

II. *Experiments on the Action of Potassium on Ammonia, and Observations on the Nature of these two Bodies.*

In the Bakerian Lecture, which I had the honour of reading before the Society, November 19, 1807, I mentioned that in heating potassium strongly in ammonia, I found that there was a considerable increase of volume of the gas, that hydrogen and nitrogen were produced, and that the potassium appeared to be oxidated; but this experiment, as I had not been able to examine the residuum with accuracy, I did not publish. I stated it as an evidence, which I intended to pursue more fully, of the existence of oxygen in ammonia.

In a paper read before the Royal Society last June, which they have done me the honour of printing, I have given an account of various experiments on the amalgam from ammonia, discovered by Messrs. Berzelius and Pontin, and in a note attached to this communication, I ventured to controvert an opinion of MM. Gay Lussac and Thenard, with respect to the agency of potassium and ammonia, even on their own statement of facts, as detailed in the Moniteur for May 27, 1808.

The general obscurity belonging to these refined objects of research, their importance and connection with the whole of chemical theory, have induced me since that time to apply to them no inconsiderable degree of labour and attention; and the results of my inquiries will, I trust, be found not only to confirm my former conclusions; but likewise to offer some novel views.

In the first of these series of operations on the action of potassium on ammonia, I used retorts of green glass; I then suspecting oxygen might be derived from the metallic oxides in the green glass, employed retorts of plate glass, and last of all I fastened the potassium upon trays of platina, or iron, which were introduced into the glass retorts furnished with stop-cocks. These retorts were exhausted by an excellent air-pump, they were filled with hydrogen, exhausted a second time, and then filled with ammonia from an appropriate mercurial gasholder.* In this way the gas was operated upon in a high degree of purity, which was always ascertained; and all the operations performed out of the contact of mercury, water, or any substances that could interfere with the results.

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* A representation of the instruments and apparatus is annexed. [Plate II.]

I at first employed potassium procured by electricity; but I soon substituted for it the metal obtained by the action of ignited iron upon potash, in the happy method discovered by MM. Gay Lussac and Thenard, finding that it gave the same results, and could be obtained of an uniform quality,* and in infinitely larger quantities, and with much less labour and expense.

When ammonia is brought in contact with about twice its weight of potassium at common temperatures, the metal loses its lustre and becomes white, there is a slight diminution in the volume of the gas; but no other effects are produced. The white crust examined proves to be potash, and the ammonia is found to contain a small quantity of hydrogen, usually not more than equal in volume to the metal. On heating the potassium in the gas, by means of a spirit lamp applied to the bottom of the retort, the colour of the crust is seen to change from white to a bright azure, and this gradually passes through shades of bright blue and green into dark olive. The crust and the metal then fuse together; there is a considerable effervescence, and the crust passing off to the sides, suffers the brilliant surface of the potassium to appear. When the potassium is cooled in this state, it is again covered with the white crust. By heating a second time, it swells considerably, becomes porous, and appears crystallized, and of a beautiful azure tint; the same series of phenomena, as those before described, occur in a continuation of the process, and it is finally entirely converted into the dark olive coloured substance.

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* When the potash used for procuring potassium in this operation was very pure, and the iron turnings likewise very pure and clean, and the whole apparatus free from any foreign matters, the metal produced differed very little in its properties, from that obtained by the Voltaic battery. Its lustre, ductility, and inflammability were similar. Its point of fusion and specific gravity were, however, a little higher, it requiring nearly 130° of Fahrenheit to render it perfectly fluid, and being to water as 7960 to 10,000, at 60° Fahrenheit. This I am inclined to attribute to its containing a minute proportion of iron.

In this operation, as has been stated by MM. Gay Lussac and Thenard, a gas which gives the same diminution by detonation with oxygen, as hydrogen, is evolved, and ammonia disappears.

The proportion of the ammonia which loses its elastic form, as I have found by numerous trials, varies according as the gas employed contains more or less moisture.

Thus eight grains of potassium, during its conversion into the olive-coloured substance, in ammonia saturated with water at 63° Fahrenheit, and under a pressure equal to that of 29·8 inches of mercury, had caused the disappearance of twelve cubical inches and a half of ammonia; but the same quantity of metal acted upon under similar circumstances, except that the ammonia had been deprived of as much moisture as possible by exposure for two days to potash that had been ignited, occasioned a disappearance of sixteen cubical inches of the volatile alkali.

Whatever be the degree of moisture of the gas, the quantities of inflammable gas generated have always appeared to me to be equal for equal quantities of metal. MM. Gay Lussac and Thenard are said to have stated, that the proportions in their experiment were the same as would have resulted from the action of water upon potassium. In my trials, they have been rather less. Thus, in an experiment conducted with every possible attention to accuracy of manipulation, eight grains of potassium generated, by their operation upon water, eight cubical inches and a half of hydrogen gas: and eight grains from the same mass, by their action upon ammonia, produced eight cubical inches and one-eighth of inflammable gas. This difference is inconsiderable, yet I have always found it to exist, even in cases where the ammonia has been in great excess, and every part of the metal apparently converted into the olive-coloured substance.

No other account of the experiments of MM. Gay Lussac and Thenard has, I believe, as yet been received in this country, except that in the Moniteur already referred to; and in this no mention is made of the properties of the substance produced by the action of ammonia on potassium. Having examined them minutely and found them curious, I shall generally describe them.

1. It is crystallized and presents irregular facets, which are extremely dark, and in colour and lustre not unlike the protoxide of iron; it is opaque when examined in large masses, but is semi-transparent in thin films, and appears of a bright brown colour by transmitted light.

2. It is fusible at a heat a little above that of boiling water, and if heated much higher, emits globules of gas.

3. It appears to be considerably heavier than water, for it sinks rapidly in oil of sassafras.

4. It is a non-conductor of electricity.

5. When it is melted in oxygen gas, it burns with great vividness, emitting bright sparks. Oxygen is absorbed, nitrogen is emitted, and potash, which from its great fusibility seems to contain water, is formed.

6. When brought in contact with water, it acts upon it with much energy, produces heat, and often inflammation, and evolves ammonia. When thrown upon water, it disappears with a hissing noise, and globules from it often move in a state of ignition upon the surface of the water. It rapidly effervesces and deliquesces in air, but can be preserved under naphtha, in which, however, it softens slowly, and seems partially to dissolve. When it is plunged under water filling an inverted jar, by means of a proper tube, it disappears instantly with effervescence, and the non-absorbable elastic fluid liberated is found to be hydrogen gas.

By far the greatest part of the ponderable matter of the ammonia, that disappears in the experiment of its action upon potassium, evidently exists in the dark fusible product. On weighing a tray containing six grains of potassium, before and after the process, the volatile alkali employed having been very dry, I found that it had increased more than two grains; the rapidity with which the product acts upon moisture, prevented me from determining the point with great minuteness; but I doubt not, that the weight of the olive-coloured substance and of the hydrogen disengaged precisely equals the weight of the potassium, and ammonia consumed.

MM. Gay Lussac and Thenard* are said to have procured from the fusible substance, by the application of a strong heat, two-fifths of the quantity of ammonia that had disappeared in their first process, and a quantity of hydrogen and nitrogen in the proportions in which they exist in ammonia, equal to one-fifth more.

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* No notice is taken of the apparatus used by MM. Gay Lussac and Thenard in the Moniteur; but, from the tenor of the details, it seems that they must have operated in glass vessels in the way heretofore adopted over mercury.

My results have been very different, and the reasons will, I trust, be immediately obvious.

When the retort containing the fusible substance is exhausted, filled with hydrogen and exhausted a second time, and heat gradually applied, the substance soon fuses, effervesces, and, as the heat increases, gives off a considerable quantity of elastic fluid, and becomes at length, when the temperature approaches nearly to dull redness, a dark gray solid, which, by a continuance of this degree of heat, does not undergo any alteration.

In an experiment, in which eight grains of potassium had absorbed sixteen cubical inches of well dried ammonia in a glass retort, the fusible substance gave off twelve cubical inches and half of gas, by being heated nearly to redness, and this gas analysed, was found to consist of three quarters of a cubical inch of ammonia, and the remainder of elastic fluids, which when mixed with oxygen gas in the proportion of 6½ to 6, and acted upon by the electric spark diminished to 5½. The temperature of the atmosphere, in this process, was 57° Fahrenheit, and the pressure equalled that of 30·1 inches of mercury.

In a similar experiment, in which the platina tray containing the fusible substance was heated in a polished iron tube, filled with hydrogen gas, and connected with a pneumatic apparatus containing very dry mercury, the quantity of elastic fluid given off, all the corrections being made, equalled thirteen cubical inches and three quarters, and of these a cubical inch was ammonia; and the residual gas, and the gas introduced into the tube being accounted for, it appeared that the elastic fluid generated, destructible by detonation with oxygen, was to the indestructible elastic fluid, as 2·5 to 1.

In this process, the heat applied approached to the dull red heat. The mercury, in the thermometer, stood at 62° Fahrenheit, and that in the barometer at 30·3 inches.

In various experiments on different quantities of the fusible substance, in some of which the heat was applied to the tray in the green glass retort, and in others, after it had been introduced into the iron tube; and in which the temperature was sometimes raised slowly and sometimes quickly, the comparative results were so near these that I have detailed, as to render any statement of them superfluous.

A little more ammonia, and rather a larger proportion of inflammable gas,* were in all instances evolved when the iron tube was used, which I am inclined to attribute to the following circumstances. When the tray was brought through the atmosphere to be introduced into the iron tube, the fusible substance absorbed a small quantity of moisture from the air, which is connected with the production of ammonia. And in the process of heating in the retort, the green glass was blackened, and I found that it contained a very small quantity of the oxides of lead and iron, which must have caused the disappearance of a small quantity of hydrogen

MM. Gay Lussac and Thenard, it appears from the statement, had brought the fusible substance in contact with mercury, which must have given to it some moisture; and whenever this is the case, it furnishes by heat variable quantities of ammonia. In one instance, in which I heated the fusible substance from nine grains of potassium, in a retort that had been filled with mercury in its common state of dryness, I obtained seven cubical inches of ammonia as the first product; and in another experiment which had been made with eight grains, and in which moisture was purposely introduced, I obtained nearly nine cubical inches of ammonia, and only four of the mixed gases.

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* The average of six experiments made in a tube of iron, is 2·4 of inflammable gas to 1 of uninflammable. The average of three made in green glass retorts, is 2·3 to 1.

I am inclined to believe, that if moisture could be introduced only in the proper proportion, the quantity of ammonia generated, would be exactly equal to that which disappeared in the first process.

This idea is confirmed by the trials which I have made, by heating the fusible substance with potash, containing its water of crystallization, and muriate of lime partially dried.*

In both these cases, ammonia was generated with great rapidity, and no other gas, but a minute quantity of inflammable gas, evolved, which was condensed by detonation with oxygen with the same phenomena as pure hydrogen.

In one instance, in which thirteen cubical inches of ammonia had disappeared, I obtained nearly eleven and three quarters by the agency of the water of the potash; the quantity of inflammable gas generated was less than four-tenths of a cubical inch.

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* If water, in its common form, is brought in contact with the fusible substance, it is impossible to regulate the quantity, so as to gain conclusive results, and a very slight *excess* of water causes the disappearance of a very large quantity of the ammonia generated. In potash and muriate of lime, in certain states of dryness, the water is too strongly attracted by the saline matter to be given off, except for the purpose of *generating* the ammonia.

In another, in which fourteen cubical inches had been absorbed, I procured by the operation of the moisture of muriate of lime, nearly eleven cubical inches of volatile alkali, and half a cubical inch of inflammable gas; and the differences, there is every reason to believe, were owing to an excess of water in the salts, by which some of the gas was absorbed.

Whenever, in experiments on the fusible substance, it has been procured from ammonia saturated with moisture, I have always found that more ammonia is generated from it by mere heat; and the general tenor of the experiments incline me to believe, that the small quantity, produced in experiments performed in vacuo, is owing to the small quantity of moisture furnished by the hydrogen gas introduced, and that the fusible substance, heated out of the presence of moisture, is incapable of producing volatile alkali.

MM. Gay Lussac and Thenard, it is stated, after having obtained three-fifths of the ammonia or its elements that had disappeared in their experiment, by heating the product; procured the remaining two-fifths, by adding water to the residuum, which after this operation was found to be potash. No notice is taken of the properties of this residuum, which, as the details seem to relate to a single experiment, probably was not examined; nor as moisture was present at the beginning of their operations could any accurate knowledge of its nature have been gained.

I have made the residuum of the fusible substance after it has been exposed to a dull red heat, out of the contact of moisture, an object of particular study, and I shall detail its general properties.

It was examined under naphtha, as it is instantly destroyed by the contact of air.

1. Its colour is black, and its lustre not much inferior to that of plumbago.

2. It is opaque even in the thinnest films.

3. It is very brittle, and affords a deep gray powder.

4. It is a conductor of electricity.

5. It does not fuse at a low red heat, and when raised to this temperature, in contact with plate glass, it blackens the glass, and a grayish sublimate rises from it, which likewise blackens the glass.

6. When exposed to air at common temperatures, it usually takes fire immediately, and burns with a deep red light.

7. When it is acted upon by water, it heats, effervesces most violently, and evolves volatile alkali, leaving behind nothing but potash. When the process is conducted under water, a little inflammable gas is found to be generated. A residuum of eight grains giving in all cases about $\frac{20}{100}$ of a cubical inch.

8. It has no action upon quicksilver.

9. It combines with sulphur and phosphorus by heat, without any vividness of effect, and the compounds are highly inflammable, and emit ammonia, and the one phosphuretted and the other sulphuretted hydrogen gas, by the action of water.

As an inflammable gas alone, having the obvious properties of hydrogen is given off during the action of potassium upon ammonia, and as nothing but gases apparently the same as hydrogen and nitrogen, nearly in the proportions in which they exist in volatile alkali, are evolved during the exposure of the compound to the degree of heat which I have specified; and as the residual substance produces ammonia with a little hydrogen by the action of water, it occurred to me, that, on the principles of the antiphlogistic theory, it ought to be a compound of potassium, a little oxygen and nitrogen, or a combination of a suboxide of potassium and nitrogen; for the hydrogen disengaged in the operations of which it was the result, nearly equalled the whole quantity contained in the ammonia employed; and it was easy to explain the fact of the reproduction of the ammonia by water, on the supposition, that by combination with one portion of the oxygen of the water, the oxide of potassium became potash, and by combination with another portion and its hydrogen, the nitrogen was converted into volatile alkali.

With a view to ascertain this point, I made several experiments on various residuums, procured in the way that I have just stated, from the action of equal quantities of potassium on dry ammonia in platina trays, each portion of metal equalling six grains.

In the first trials, I endeavoured to ascertain the quantity of ammonia generated by the action of water upon a residuum, by heating it with muriate of lime or potash partially deprived of moisture; and after several trials, many of which failed, I succeeded in obtaining four cubical inches and a half of ammonia. In three other cases, where there was reason to suspect a small excess of water, the quantities of ammonia were three cubical inches and a half, three and eight-tenths, and four and two-tenths.

These experiments were performed in the iron tube used for the former process; the tray was not withdrawn; but the salt introduced in powder, and the apparatus exhausted as before, then filled with hydrogen, and then gently heated in a small portable forge.

Having ascertained what quantity of ammonia was given off from the residuum, I endeavoured to discover what quantity of nitrogen it produced in combustion, and what quantity of oxygen it absorbed. The methods that I employed, were by introducing the trays into vessels filled with oxygen gas over mercury. The product often inflamed spontaneously, and could always be made to burn by a slight degree of heat.

In the trial that I regard as the most accurate, two cubical inches and a half of oxygen were absorbed, and only a cubical inch and one-tenth of nitrogen evolved.

Surprised at the smallness of the quantity of the nitrogen, I sought for ammonia in the products of these operations; but various trials convinced me that none was formed. I examined the solid substances produced, expecting nitrous acid; but the matter proved to be dry potash, apparently pure, and not affording the slightest traces of acid.

The quantity of nitrogen existing in the ammonia, which this residuum would have produced by the action of *water*, supposing the volatile alkali decomposed by electricity, would have equalled at least two cubical inches and a quarter.

I heated the same proportions of residuum with the red oxide of mercury, and the red oxide of lead in vacuo, expecting that when oxygen was supplied in a gradual way, the result might be different from that of combustion; but in neither of these cases did the quantity of nitrogen exceed a cubical inch and a half.

But on what could this loss of nitrogen depend; had it entered into any unknown form with oxygen, or did it not really exist in the residuum in the same quantity, as in the *ammonia* produced from it?

I hoped that an experiment of exposing the residuum to intense heat might enlighten the inquiry. I distilled one of the portions which had been covered with naphtha, in a tube of wrought platina made for the purpose. The tube had been exhausted and filled with hydrogen, and exhausted again, and was then connected with a pneumatic mercurial apparatus. Heat was at first slowly applied till the naphtha had been driven over. It was then raised rapidly by an excellent forge. When the tube became cherry red, gas was developed; it continued to be generated for some minutes. When the tube had received the most intense heat that could be applied, the operation was stopped. The quantity of gas collected, making the proper corrections and reductions, would have been three cubical inches and a half at the mean temperature and pressure. Twelve measures of it were mixed with six of oxygen gas, the electrical spark was passed through the mixture; a strong inflammation took place, the diminution was to three measures and a half, and the residuum contained oxygen. This experiment was repeated upon different quantities with the same comparative results.

In examining the platina tube, which had a screw adapted to it at the lower extremity, by means of which it could be opened, the lower part was found to contain potash, which had all the properties of the pure alkali, and in the upper part there was a quantity of potassium. Water poured into the tube produced a violent heat and inflammation; but no smell of ammonia.

This result was so unexpected and so extraordinary, that I at first supposed there was some source of error. I had calculated upon procuring nitrogen as the only aëriform product; I obtained an elastic fluid which gave much more diminution by detonation with oxygen, than that produced from ammonia by electricity.

I now made the experiment, by heating the entire fusible substance, from six grains of potassium which had absorbed twelve cubical inches of ammonia, in the iron tube, in the manner before described. The heat was gradually raised to whiteness, and the gas collected in two portions. The whole quantity generated, making the usual corrections for temperature and pressure, and the portion of hydrogen originally in the tube, and the residuum, would have been fourteen cubical inches and a half at the mean degree of the barometer and thermometer. Of these, nearly a cubical inch was ammonia and the remainder a gas, of which the portion destructible by detonation with oxygen, was to the indestructible portion, as 2·7 to 1.

The lower part of the tube, where the heat had been intense, was found surrounded with potash in a vitreous form; the upper part contained a considerable quantity of potassium.

In another similar experiment, made expressly for the purpose of ascertaining the quantity of potassium recovered, the same elastic products were evolved. The tube was suffered to cool, the stop-cock being open in contact with mercury, it was filled with mercury, and the mercury displaced by water; when two cubical inches and three quarters of hydrogen gas were generated, which proved that at least two grains and a half of potassium had been revived.

Now, if a calculation be made upon the products in these operations, considering them as nitrogen and hydrogen, and taking the common standard of temperature and pressure, it will be found, that by the decomposition of 11 cubical inches of ammonia equal to 2·05 grains, there is generated 3·6 cubical inches of nitrogen equal to 1·06 grains, and 9·9 cubical inches of hydrogen, which added to that disengaged in the first operation equal to about 6·1 cubical inches, are together equal to ·382 grains; and the oxygen added to 3·5 grains of potassium would be ·6 grains, and the whole amount is 2·04 grains; and 20·5—20·4 = ·01. But the same quantity of ammonia, decomposed by electricity, would have given 5·5 cubical inches of nitrogen equal to 1·6 grains, and only 14 cubical inches of hydrogen* equal to ·33, and allowing the separation of oxygen in this process in water, it cannot be estimated at more than ·11 or ·12.

So that if the analysis of ammonia by electricity at all approaches towards accuracy; in the process just described, there is a considerable loss of nitrogen, and a production of oxygen and inflammable gas.

And in the action of water upon the residuum, in the experiment page 153, there is an apparent generation of nitrogen.

How can these extraordinary results be explained?

The decomposition and composition of nitrogen seem proved, allowing the correctness of the data; and one of its elements appears to be oxygen; but what is its other elementary matter? †

Is the gas that appears to possess the properties of hydrogen, a new species of inflammable aëriform substance?

Or has nitrogen a metallic basis which alloys with the iron or platina?

Or is water alike the *ponderable* matter of nitrogen, hydrogen, and oxygen?

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* See Phil. Trans. 1808, p. 40.

† [The results of the author's later researches on this extremely difficult subject, were not favorable to the above conclusion, they were chiefly negative: vide Section 3 of the Bakerian Lecture for the following year (1809) for the details.]

Or is nitrogen a compound of hydrogen with a larger proportion of oxygen than exists in water?

These important questions, the two first of which seem the least likely to be answered in the affirmative, from the correspondence between the weight of the ammonia decomposed, and the products, supposing them to be known substances, I shall use every effort to solve by new labours, and I hope soon to be able to communicate the results of further experiments on the subject to the Society.

As the inquiry now stands, it is however sufficiently demonstrative, that the opinion which I had ventured to form respecting the decomposition of ammonia in this experiment, is correct; and that MM. Gay-Lussac's and Thenard's idea of the decomposition of the potassium, and their theory of its being compounded of hydrogen and potash, are unfounded.

For a considerable part of the potassium is recovered unaltered, and in the entire decomposition of the fusible substance, there is only a small excess of hydrogen above that existing in the ammonia acted upon.

The mere phenomena of the process likewise, if minutely examined, prove the same thing.

After the first slight effervescence, owing to the water absorbed by the potash formed upon the potassium during its exposure to the air, the operation proceeds with the greatest tranquillity. No elastic fluid is given off from the potassium; it often appears covered with the olive-coloured substance, and if it were evolving hydrogen, this must pass through the fluid; but even to the end of the operation, no such appearance occurs.

The crystallized and spongy substance, formed in the first part of the process, I am inclined to consider as a combination of ammonium and potassium, for it emits a smell of ammonia when exposed to air, and is considerably lighter than potassium.

I at first thought that a solid compound of hydrogen and potassium might be generated in the first part of this operation: but experiments on the immediate action of potassium and hydrogen did not favour this opinion. Potassium, as I ventured to conclude in the Bakerian Lecture for 1807,* is very soluble in hydro-

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* MM. Gay Lussac and Thenard seem to be of a different opinion. In the Moniteur, to which I have so often referred, it is related, that these distinguished chemists, exposing hydrogen to potassium at a high temperature, found that the hydrogen was absorbed, and that it formed a compound with the potassium of a light gray colour, from which hydrogen was capable of being obtained by the action of water or mercury.

After a number of trials, I have not been able to witness this result. In an experiment which I made in the presence of Mr. Pepys, and which I have often repeated, and twice before a numerous assembly, in retorts of plate glass, four grains of potassium were heated in fourteen cubical inches of pure hydrogen. At first, white fumes arose and precipitated themselves in the neck of the retort. When a considerable film of the precipitate had collected, its colour appeared a bright gray, and after the first two or three minutes, it ceased to be formed.

The bottom of the retort was heated to redness, when the potassium began to sublime and condense on the sides.

The process was stopped, and the retort suffered to cool. The absorption was not equal to a quarter of a cubical inch. When the retort was *broken*, the gas in passing into the atmosphere, produced an explosion with most vivid light, and white fumes. The potassium remaining in the retort, and that which had sublimed, seemed unaltered in their properties.

The grayish substance inflamed by the action of water, but did not seem to be combinable with mercury. I am inclined to attribute its formation to the agency of moisture suspended in the hydrogen, and to consider it as a triple compound of potassium, oxygen, and hydrogen.

When potassium is heated in a gas containing hydrogen, and from $\frac{1}{12}$ to $\frac{1}{30}$ of common air, it is formed in greater quantities, and a crust of it covers the metal, and in the process there is an absorption both of hydrogen and oxygen. It is likewise produced in experiments on the generation of potassium by exposing potash to ignited iron, at the time gen; but, under common circumstances, hydrogen does not seem to be absorbable by potassium.

(I believe) that common air is admitted, during the cooling of the tube.

It is non-conducting, inflames spontaneously in air, and produces potash and aqueous vapour by its combustion.

When potassium is heated in hydrogen in a flint glass retort, or even for a great length of time in a green glass retort, there is an absorption of the gas; but this is independent of the presence of potassium, and is owing to the action of the metallic oxides in the glass upon the hydrogen.

If a solid compound of hydrogen and potassium could be formed, we might expect its existence in the experiment with the gun barrel, in which potassium is exposed to hydrogen at almost every temperature; but the metal formed in this process, when proper precautions are taken to exclude carbonaceous matters, is uniform in its properties, and generates for equal quantities, equal proportions of hydrogen by the action of water.

The general phenomena of this operation, shew indeed that the solution of potassium in hydrogen is intimately connected with the general principle of the decomposition, and confirm my first idea of the action of the two bodies.

Hydrogen dissolves a large quantity of potassium by heat, but the greater portion is precipitated on cooling. The attractions which determine the chemical change, seem to be that of iron for oxygen, of iron for potassium, and of hydrogen for potassium; and in experiments, in which a very intense heat is used for the production of potassium by iron, I have often found, that the gas which comes over, though it has passed through a tube cooled by ice, inflames spontaneously in the atmosphere, and burns with a most brilliant light which is purple at the edges, and throws off a dense vapour containing potash.

Sodium appears to be almost insoluble in hydrogen, and this seems to be one reason why it cannot be obtained, except in very minute quantities, in the experiment with the gun-barrel.

Sodium, though scarcely capable of being dissolved in hydrogen alone, seems to be soluble in the compound of hydrogen and potassium. By exposing mixtures of potash and soda, to ignited iron I have obtained some very curious alloys; which, whether the potassium or the sodium was in excess, were fluid at common temperatures. The compound containing an excess of potassium was even lighter than potassium (probably from its fluidity.) All these alloys were in the highest degree inflam-

III. *Analytical Experiments on Sulphur.*

I have referred, on a former occasion,* to the experiments of Mr. Clayfield and of M. Berthollet, jun. which seemed to shew that sulphur, in its common form, contained hydrogen. In considering the analytical powers of the Voltaic apparatus, it occurred to me, that though sulphur, from its being a non-conductor, could not be expected to yield its elements to electrical attractions and repulsions of the opposite surfaces, yet that the intense heat, connected with the contact of these surfaces, might possibly effect some alteration in it, and tend to separate any elastic matter it might contain.

On this idea some experiments were instituted in 1807. A curved glass tube, having a platina wire hermetically sealed in its upper extremity, was filled with sulphur. The sulphur was melted over a spirit lamp; and a proper connection being made with the Voltaic apparatus of one hundred plates of six inches, in great activity, a contact was made in the sulphur by means of another platina wire. A most brilliant spark, which appeared orange-coloured through the sulphur, was produced, and a minute portion of elastic fluid rose to the upper extremity of the tube. By a continuation of the process for nearly an hour, a globule equal to about the tenth of an inch in diameter was obtained, which, when examined, was found to be sulphuretted hydrogen.

This result perfectly coincided with those which have

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mable. When a globule of the fluid alloy was touched by a globule of mercury, they combined with a heat that singed the paper upon which the experiment was made, and formed, when cool, a solid so hard, as not to be cut by a knife.

* Bakerian Lecture, 1808, p. 73.

been just mentioned; but as the sulphur that I had used was merely in its common state, and as the ingenious experiments of Dr. Thomson have shewn that sulphur in certain forms may contain water, I did not venture, at that time, to form any conclusion upon the subject.

In the summer of the present year, I repeated the experiment with every precaution. The sulphur that I employed was Sicilian sulphur, that had been recently sublimed in a retort filled with nitrogen gas, and that had been kept hot till the moment that it was used. The power applied was that of the battery of five hundred double plates of six inches, highly charged. In this case the action was most intense, the heat strong, and the light extremely brilliant; the sulphur soon entered into ebullition, elastic matter was formed in great quantities, much of which was permanent; and the sulphur from being of a pure yellow, became of a deep red brown tint.

The gas, as in the former instance, proved to be sulphuretted hydrogen. The platina wires were considerably acted upon; the sulphur, at its point of contact with them, had obtained the power of reddening moistened litmus paper.

I endeavoured to ascertain the quantity of sulphuretted hydrogen evolved in this way from a given quantity of sulphur, and for this purpose, I electrized a quantity equal to about two hundred grains in an apparatus of the kind I have just described, and when the upper part of the tube was full of gas, I suffered it to pass into the atmosphere; so as to enable me to repeat the process.

When I operated in this way, there seemed to be no limit to the generation of elastic fluid, and in about two hours a quantity had been evolved, which amounted to more than five times the volume of the sulphur employed. From the circumstances of the experiment, the last portion only could be examined, and this proved to be sulphuretted hydrogen. Towards the end of the process, the sulphur became extremely difficult of fusion, and almost opaque, and when cooled and broken, was found of a dirty brown colour.

The experiments upon the union of sulphur and potassium, which I laid before the Society last year, prove that these bodies act upon each other with great energy, and that sulphuretted hydrogen is evolved in the process, with intense heat and light.

In heating potassium in contact with compound inflammable substances, such as resin, wax, camphor, and fixed oils in close vessels out of the contact of the air, I found that a violent inflammation was occasioned, that hydrocarbonate was evolved; and that when the compound was not in great excess, a substance was formed, spontaneously inflammable at common temperatures, the combustible materials of which were charcoal and potassium.

Here was a strong analogy between the action of these bodies, and sulphur on potassium. Their physical properties likewise resemble those of sulphur; for they agree in being non-conductors, whether fluid or solid, in being transparent when fluid, and semi-transparent when solid, and highly refractive; their affections by electricity are likewise similar to those of sulphur; for the oily bodies give out hydrocarbonate by the agency of the Voltaic spark, and become brown, as if from the deposition of carbonaceous matter.

But the resinous and oily substances are compounds of a small quantity of hydrogen and oxygen, with a large quantity of a carbonaceous basis. The existence of hydrogen in sulphur is fully proved, and we have no right to consider a substance, which can be produced from it in such large quantities, merely as an accidental ingredient.

The oily substances in combustion, produce two or three times their weight of carbonic acid and some water; I endeavoured to ascertain whether water was formed in the combustion of sulphur in oxygen gas, dried by exposure to potash: but in this case sulphurous acid is produced in much larger quantities than sulphuric acid, and this last product is condensed with great difficulty. In cases, however, in which I have obtained, by applying artificial cold, a deposition of acid in the form of a film of dew in glass retorts out of the contact of the atmosphere, in which sulphur had been burned in oxygen gas hygrometrically dry, it has appeared to me less tenacious and lighter than the common sulphuric acid of commerce, which in the most concentrated form in which I have seen it, namely, at 1·855, gave abundance of hydrogen as well as sulphur, at the negative surface in the Voltaic circuit, and hence evidently contained water.

The reddening of the litmus paper, by sulphur that had been acted on by Voltaic electricity, might be ascribed to its containing some of the sulphuretted hydrogen formed in the process; but even the production of this gas, as will be immediately seen, is an evidence of the existence of oxygen in sulphur.

In my early experiments on potassium, procured by electricity, I heated small globules of potassium in large quantities of sulphuretted hydrogen, and I found that sulphuret of potash was formed; but this might be owing to the water dissolved in the gas, and I ventured to draw no conclusion till I had tried the experiment in an unobjectionable manner.

I heated four grains of potassium in a retort of the capacity of twenty cubical inches; it had been filled after the usual processes of exhaustion with sulphuretted hydrogen, dried by means of muriate of lime that had been heated to whiteness; as soon as the potassium fused, white fumes were copiously emitted, and the potassium soon took fire, and burnt with a most brilliant flame, yellow in the centre and red towards the circumference.*

The diminution of the volume of the elastic matter, in this operation, did not equal more than two cubical inches and a half. A very small quantity of the residual gas only was absorbed by water. The non-absorbable gas was hydrogen, holding a minute quantity of sulphur in solution.

A yellow sublimate lined the upper part of the retort, which proved to be sulphur. The solid matter formed was red at the surface like sulphuret of potash, but in the interior it was dark gray, like sulphuret of potassium. The piece of the retort containing it was introduced into a jar inverted over mercury, and acted upon by a small quantity of dense muriatic acid, diluted with an equal weight of water, when there were disengaged two cubical inches and a quarter of gas, which proved to be sulphuretted hydrogen.

In another experiment, in which eight grains of potassium were heated in a retort of the capacity of twenty cubical inches, containing about nineteen cubical inches of sulphuretted hydrogen, and a cubical inch of phosphuretted hydrogen, which was introduced for the purpose of absorbing the oxygen of the small quantity of common air admitted by the stop-cock, the inflammation took place as before, there was a similar precipitation of sulphur on the sides of the retort; the mass formed in the place of the potassium was orange externally, and of a dark gray colour internally, as in the last instance; and when acted on by a little water holding muriatic acid in solution, there were evolved from it five cubical inches only of sulphuretted hydrogen.

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* In the Moniteur, May 27, 1808, in the account of MM. Gay Lussac's and Thenard's experiments, it is mentioned, that potassium absorbs the sulphur and a part of the hydrogen of sulphuretted hydrogen; but the phenomena of inflammation is not mentioned, nor are the results described.

Both these experiments concur in proving the existence of a principle in sulphuretted hydrogen, capable of destroying partially the inflammability of potassium, and of producing upon it all the effects of oxygen; for had the potassium combined merely with pure combustible matter, it ought, as will be seen distinctly from what follows, to have evolved by the action of the acid, a volume of sulphuretted hydrogen, at least equal to that of the hydrogen, which an equal weight of uncombined potassium would have produced by its operation upon water.

Sulphuretted hydrogen, as has been long known to chemists, may be formed by heating sulphur strongly in hydrogen gas. I heated four grains of sulphur in a glass retort, containing about twenty cubical inches of hydrogen, by means of a spirit lamp, and pushed the heat nearly to redness. There was no perceptible change of volume in the gas after the process; the sulphur that had sublimed was unaltered in its properties, and about three cubical inches of an elastic fluid absorbable by water were formed: the solution reddened litmus, and had all the properties of a solution of pure sulphuretted hydrogen. Now if we suppose sulphuretted hydrogen to be constituted by sulphur dissolved in its unaltered state in hydrogen, and allow the existence of oxygen in this gas; its existence must likewise be allowed in sulphur, for we have no right to assume that sulphur in sulphuretted hydrogen is combined with more oxygen than in its common form: it is well known, that when electrical sparks are passed through sulphuretted hydrogen, a considerable portion of sulphur is separated without any alteration in the volume of the gas. This experiment I have made more than once, and I found that the sulphur obtained, in fusibility, combustibility, and other sensible properties did not perceptibly differ from common sublimed sulphur.

According to these ideas, the intense ignition produced by the action of sulphur, on potassium and sodium, must not be ascribed merely to the affinity of the metals of the alkalies for its basis, but may be attributed likewise to the agency of the oxygen that it contains.

The minute examination of the circumstances of the action of potassium and sulphur likewise confirms these opinions.

When two grains of potassium and one of sulphur were heated gently in a green glass tube filled with hydrogen, and connected with a pneumatic apparatus, there was a most intense ignition produced by the action of the two bodies, and one-eighth of a cubical inch of gas was disengaged, which was sulphuretted hydrogen. The compound was exposed in a mercurial apparatus to the action of liquid muriatic acid; when a cubical inch and a quarter of aëriform matter was produced, which proved to be pure sulphuretted hydrogen.

The same experiment was repeated, except that four grains of sulphur were employed instead of one. In this case, a quarter of a cubical inch of gas was disengaged during the process of combination; and when the compound was acted upon by muriatic acid, only three quarters of a cubical inch of sulphuretted hydrogen was obtained.

Now *sulphuret* of potash produces sulphuretted hydrogen by the action of an acid; and if the sulphur had not contained oxygen, the hydrogen evolved by the action of the potassium in both these experiments ought to have equalled at least two cubical inches, and the whole quantity of sulphuretted hydrogen ought to have been more: and that so much less sulphuretted hydrogen was evolved in the second experiment, can only be ascribed to the larger quantity of oxygen furnished to the potassium by the larger quantity of the sulphur.

I have made several experiments of this kind with similar results. Whenever equal quantities of potassium were combined with unequal quantities of sulphur, and exposed afterwards to the action of muriatic acid, the largest quantity of sulphuretted hydrogen was furnished by the product containing the smallest proportion of sulphur, and in no case was the quantity of gas equal in volume to the quantity of hydrogen, which would have been produced by the mere action of potassium upon water.

From the general tenor of these various facts, it will not be, I trust, unreasonable to assume, that sulphur, in its common state, is a compound of small quantities of oxygen* and hydrogen with a large quantity of a basis that produces the acids of sulphur in combustion, and which, on account of its strong attractions for other bodies, it will probably be very difficult to obtain in its pure form.

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* [This conclusion was not confirmed by the author's later researches: vide note in Section 5, of his Bakerian Lecture for 1809, in explanation, and the latter part of his paper, entitled "Researches on Oximuriatic Acid, &c." made in 1810.]

In metallic combinations even, it still probably retains its oxygen and part of its hydrogen. Metallic sulphurets can only be partially decomposed by heat, and the small quantity of sulphur evolved from them in this case when perfectly dry and out of the contact of air, as I found in an experiment on the sulphurets of copper and iron, exists in its common state, and acts upon potassium, and is affected by electricity in the same manner as native sulphur.

IV. *Analytical Experiments on Phosphorus.*

The same analogies apply to phosphorus as to sulphur, and I have made a similar series of experiments on this inflammable substance.

Common electrical sparks, passed through phosphorus, did not evolve from it any permanent gas; but when it was acted upon by the Voltaic electricity of the battery of five hundred plates in the same manner as sulphur, gas was produced in considerable quantities, and the phosphorus became of a deep red brown colour, like phosphorus that has been inflamed and extinguished under water. The gas examined proved to be phosphuretted hydrogen, and in one experiment, continued for some hours, a quantity estimated to be nearly equal to four times the volume of the phosphorus employed was given off. The light of the Voltaic spark in the phosphorus was at first a brilliant yellow, but as the colour of the phosphorus changed, it appeared orange.

I heated three grains of potassium in sixteen cubical inches of phosphuretted hydrogen; as soon as it was fused, the retort became filled with white fumes, and a reddish substance precipitated upon the sides and upper part of it. The heat was applied for some minutes. No inflammation took place.* When the retort was cool, the absorption was found to be less than a cubical inch. The potassium externally was of a deep brown colour, internally it was of a dull lead colour. The residual gas had lost its property of spontaneous inflammation, but seemed still to contain a small quantity of phosphorus in solution.

The phosphuret acted upon over mercury by solution of muriatic acid evolved only one cubical inch and three quarters of phosphuretted hydrogen.

From this experiment, there is great reason to suppose that phosphuretted hydrogen contains a minute proportion of oxygen, and consequently that phosphorus likewise may contain it; but the action of potassium on phosphorus itself furnishes perhaps more direct evidences of the circumstance.

One grain of potassium and one grain of phosphorus were fused together in a proper apparatus. They combined with the production of the most vivid light and intense ignition. During the process one-tenth of a cubical inch of phosphuretted hydrogen was evolved. The phosphuret formed, exposed to the action of diluted muriatic acid over mercury, produced exactly three-tenths of a cubical inch of phosphuretted hydrogen.

In a second experiment, one grain of potassium was fused with three grains of phosphorus; in this case nearly a quarter of a cubical inch of phosphuretted hydrogen was generated during the ignition. But from the compound exposed to muriatic acid, only one-tenth of a cubical inch could be procured.

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* It is stated, in the account before referred to of MM. Gay Lussac's and Thenard's experiments, that potassium inflames in phosphuretted hydrogen. My experiments upon this gas have been often repeated. I nave never perceived any luminous appearance; but I have always operated in day-light.

Now it is not easy to refer the deficiency of phosphuretted hydrogen in the second case to any other cause than to the supply of oxygen to the potassium from the phosphorus;* and the quantity of phosphuretted hydrogen evolved in the first case, is much less than could be expected, if both potassium and phosphorus consisted merely of pure combustible matter.

The phosphoric acid, formed by the combustion of phosphorus, though a crystalline solid, may still contain water. The hydrogen evolved from phosphorus by electricity proves indeed that this must be the case; and though the quantity of hydrogen and oxygen in phosphorus may be exceedingly small, yet they may be sufficient to give it peculiar characters; and till the basis is obtained free, we shall have no knowledge of the properties of the pure phosphoric element.

V. *On the States of the Carbonaceous Principle in Plumbago, Charcoal, and the Diamond.*

The accurate researches of Messrs. Allen and Pepys have distinctly proved, that plumbago, charcoal, and the diamond produce very nearly the same quantities of carbonic acid, and absorb very nearly the same quantities of oxygen in combustion.

Hence it is evident, that they must consist principally of the same kind of elementary matter; but minute researches upon their chemical relations, when examined by new analytical methods will, I am inclined to believe, shew that the great difference in their physical properties does not merely depend upon the differences of the mechanical arrangement of their parts, but likewise upon differences in their intimate chemical nature.

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* [The remark in page 167, relative to the existence of oxygen and hydrogen in sulphur is applicable to phosphorus, as is also the reference.]

I endeavoured to discover, whether any elastic matter could be obtained from plumbago very intensely ignited by the Voltaic battery in a Torricellian vacuum: but though the highest power of the battery of 500 was employed, and though the heat was such, as in another experiment instantly melted platina wire of $\frac{1}{60}$ of an inch in diameter, yet no appearance of change took place upon the plumbago. Its characters remained wholly unaltered, and no permanent elastic fluid was formed.

I heated one grain of plumbago, with twice its weight of potassium, in a plate glass tube connected with a proper apparatus, and I heated an equal quantity of potassium alone in a tube of the same kind, for an equal length of time, namely, eight minutes. Both tubes were filled with hydrogen: no gas was evolved in either case. There was no ignition in the tube containing the plumbago, but it seemed gradually to combine with the potassium. The two results were exposed to the action of water; the result from the plumbago acted upon that fluid with as much energy as the other result, and the two volumes of elastic fluids were 1·8 cubical inches and 1·9 cubical inches; and both gave the same diminution by detonation with oxygen, as pure hydrogen. Two grains of potassium, by acting upon water, would have produced two cubical inches and one-eighth of hydrogen gas; the deficiency in the result, in which potassium alone was used, must be ascribed to the loss of a small quantity of metal, which must have been carried off in solution in the hydrogen, and perhaps, likewise, to the action of the minute quantity of metallic oxides in the plate glass. The difference in the quantity of hydrogen given off in the two results, is however too slight to ascribe it to the existence of oxygen in the plumbago.

I repeated this experiment several times with like results, and in two or three instances examined the compound formed. It was infusible at a red heat, had the lustre of plumbago. It inflamed spontaneously, when exposed to air, generated potash, and left a black powdery residuum. It effervesced most violently in water, and produced a gas, which burnt like pure hydrogen.

When small pieces of charcoal from the willow, that had been intensely ignited, were acted upon by Voltaic electricity in a Torricellian vacuum, every precaution being taken to exclude moisture from the mercury and the charcoal, the results were very different from those occurring in the case of plumbago.

When plumbago was used, after the first spark, which generally passed through a distance of about one-eighth of an inch, there was no continuation of light, without a contact or an approach to the same distance; but from the charcoal a flame seemed to issue of a most brilliant purple, and formed, as it were, a conducting chain of light of nearly an inch in length, at the same time that elastic matter was rapidly formed, some of which was permanent. After many unsuccessful trials, I at length succeeded in collecting the quantity of elastic fluid given out by half a grain of charcoal; the process had been continued nearly half an hour. The quantity of gas amounted to nearly an eighth of a cubical inch: it was inflammable by the electric spark with oxygen gas, and four measures of it absorbed three measures of oxygen, and produced one measure and a half of carbonic acid. The charcoal in this experiment had become harder at the point, and its lustre, where it had been heated to whiteness, approached to that of plumbago.

I heated two grains of potassium together with two grains of charcoal, for five minutes; and to estimate the effects of the metallic oxides and potash in the green glass tube, I made a comparative experiment, as in the case of plumbago; but there was no proof of any oxygen being furnished to the potassium from the charcoal in the process, for the compound acted upon water with great energy, and produced a quantity of inflammable gas, only inferior by one-twelfth to that produced by the potassium, which had not been combined with charcoal, and which gave the same diminution by detonation with oxygen; and the slight difference may be well ascribed to the influence of foreign matters in the charcoal. There was no ignition in the process, and no gas was evolved.

The compound produced in other experiments of this kind was examined. It is a conductor of electricity, is of a dense black, inflames spontaneously, and burns with a deep red light in the atmosphere.*

The non-conducting nature of the diamond and its infusibility, rendered it impossible to act upon it by Voltaic electricity; and the only new agents which sèemed to offer any means of decomposing it, were the metals of the alkalies.

When a diamond is heated in a green glass tube with potassium, there is no elastic fluid given out, and no intensity of action; but the diamond soon blackens, and scales seem to detach themselves from it, and these scales, when examined in the magnifier, are gray externally, and of the colour of plumbago internally, as if they consisted of plumbago covered by the gray oxide of potassium.

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* In the Bakerian Lecture for 1807, I have mentioned the decomposition of carbonic acid by potassium, which takes place with inflammation. If the potassium is in excess in this experiment, the same pyrophorus as that described above is formed.

In heating together three grains of diamonds in powder, and two grains of potassium, for an hour in a small retort of plate glass filled with hydrogen, and making the comparative trial with two grains of potassium heated in a similar apparatus, without any diamonds, I found that the potassium which had been heated with the diamonds, produced, by its action upon water, one cubical inch and $\frac{3}{10}$ of inflammable air, and that which had been exposed to heat alone, all other circumstances being similar, evolved nearly one cubical inch and $\frac{7}{10}$, both of which were pure hydrogen.

In another experiment of a similar kind, in which fragments of diamonds were used in the quantity of four grains, the potassium became extremely black from its action upon them during an exposure to heat for three hours, and the diamonds were covered with a greyish crust, and when acted upon by water and dried, were found to have lost about $\frac{28}{100}$ of a grain in weight. The matter separated by washing, and examined, appeared as a fine powder of a dense black colour. When a surface of platina wire was covered with it, and made to touch another wire in the Voltaic circuit, a brilliant spark with combustion occurred. It burnt, when heated to redness in a green glass tube filled with oxygen gas, and produced carbonic acid by its combustion.

These general results seem to shew, that in plumbago the carbonaceous element exists merely in combination with iron, and in a form which may be regarded as approaching to that of a metal in its nature, being conducting in a high degree, opaque, and possessing considerable lustre.

Charcoal appears to contain a minute quantity of hydrogen in combination. Possibly likewise, the alkalies and earths produced during its combustion, exist in it not fully combined with oxygen, and according to these ideas, it is a very compounded substance, though in the main it consists of the pure carbonaceous element.

The experiments on the diamond render it extremely likely that it contains oxygen; but the quantity must be exceedingly minute, though probably sufficient to render the compound non-conducting: and if the carbonaceous element in charcoal and the diamond be considered as united to still less foreign matter in quantity, than in plumbago, which contains about $\frac{1}{20}$ of iron, the results of their combustion, as examined independently of hygrometrical tests, will not differ perceptibly.

Whoever considers the difference between iron and steel, in which there does not exist more than $\frac{1}{200}$ of plumbago, or the difference between the amalgam of ammonia, and mercury, in which the quantity of new matter is not more than $\frac{1}{12000}$, or that between the metals and their suboxides, some of which contain less than $\frac{1}{10}$ of oxygen, will not be disposed to question the principle, and minute differences in chemical composition may produce great differences in external and physical characters.*

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* [Vide Vol. I. p. 182, and the author's later researches on the above subjects, in a paper, entitled "Some Experiments on the Combustion of the Diamond and other Carbonaceous Substances,"—the principal results of which were of a negative kind, and not in accordance with his expectations.]

VI. *Experiments on the Decomposition and Composition of the Boracic Acid.*

In the last Bakerian Lecture,* I have given an account of an experiment in which boracic acid appeared to be decomposed by Voltaic electricity, a dark coloured inflammable substance separating from it on the negative surface.

In the course of the spring and summer, I made many attempts to collect quantities of this substance for minute examination. When boracic acid, moistened with water, was exposed between two surfaces of platina, acted on by the full power of the battery of five hundred, an olive-brown matter immediately began to form on the negative surface, which gradually increased in thickness, and at last appeared almost black. It was permanent in water, but soluble with effervescence in warm nitrous acid. When heated to redness upon the platina it burnt slowly, and gave off white fumes, which slightly reddened moistened litmus paper, and it left a black mass, which, when magnified by the magnifier, appeared vitreous at the surface, and evidently contained a fixed acid.

These circumstances seemed distinctly to show the decomposition, and recomposition of the boracic acid; but as the peculiar combustible substance was a non-conductor of electricity, I was never able to obtain it, except in very thin films upon the platina. It was not possible to examine its properties minutely, or to determine its precise nature, or whether it was the pure boracic basis; I consequently endeavoured to apply other methods of decomposition, and to find other more unequivocal evidences upon this important chemical subject.

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* Page 100.

I have already laid before the Society an account of an experiment,* in which boracic acid, heated in contact with potassium in a gold tube, was converted into borate of potash, at the same time that a dark coloured matter, similar to that produced from the acid by electricity, was formed. About two months after this experiment had been made, namely in the beginning of August, at a time that I was repeating the process, and examining minutely the results, I was informed, by a letter from Mr. Cadell at Paris, that M. Thenard was employed in the decomposition of the boracic acid by potassium, and that he had heated the two substances together in a copper tube, and had obtained borate of potash, and a peculiar matter concerning the nature of which no details were given in the communication.

That the same results must be obtained by the same methods of operating, there could be no doubt. The evidences for the decomposition of the boracic acid are easily gained, the synthetical proofs of its nature involve more complicated circumstances.

I found that when equal weights of potassium and boracic acid were heated together in a green glass tube, which had been exhausted after having been twice filled with hydrogen, there was a most intense ignition before the temperature was nearly raised to the red heat; the potassium entered into vivid inflammation, where it was in contact with the boracic acid. When this acid had been heated to whiteness, before it was introduced into the tube, and powdered and made use of whilst yet warm, the quantity of gas given out in the operation did not exceed twice the volume of the acid, and was hydrogen.

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* Page 113.

I could only use twelve or fourteen grains of each of the two substances in this mode of conducting the experiment: for when larger quantities were employed, the glass tube always ran into fusion from the intensity of the heat produced during the action.

When the film of naphtha had not been carefully removed from the potassium, the mass appeared black throughout; but when this had been the case, the colour was of a dark olive-brown.

In several experiments, in which I used equal parts of the acid and metal, I found that there was always a great quantity of the former in the residuum, and by various trials, I ascertained that twenty grains of potassium had their inflammability entirely destroyed by about eight grains of boracic acid.

For collecting considerable portions of the matters formed in the process, I used metallic tubes furnished with stop-cocks, and exhausted after being filled with hydrogen.

When tubes of brass or copper were employed, the heat was only raised to a dull red; but when iron tubes were used, it was pushed to whiteness. In all cases the acid was decomposed, and the products were scarcely different.

When the result was taken out of a tube of brass or copper, it appeared as an olive-coloured glass, having opaque, dull olive-brown specks diffused through it.

It gave a very slight effervescence with water, and partially dissolved in hot water, a dark olive-coloured powder separating from it.

The results from the iron tube, which had been much more strongly heated, were dark olive in some parts, and almost black in others. They did not effervesce with warm water, but were rapidly acted upon by it, and the particles separated by washing, were of a shade of olive, so dark as to appear almost black on white paper.

The solutions obtained, when passed through a filter, had a faint olive tint, and contained sub-borate of potash, and potash. In cases, when instead of water, a weak solution of muriatic acid was used for separating the saline matter, from the inflammable matter, the fluid came through the filter colourless.

In describing the properties of the new inflammable substance separated by washing, I shall speak of that collected from operations conducted in tubes of brass, in the manner that has been just mentioned: for it is in this way, that I have collected the largest quantities.

It appears as a pulverulent mass of the darkest shades of olive. It is perfectly opaque. It is very friable, and its powder does not scratch glass. It is a non-conductor of electricity.

When it has been dried only at 100° or 200°, it gives off moisture by increase of temperature, and if heated in the atmosphere, takes fire at a temperature below the boiling point of olive-oil, and burns with a red light and scintillations like charcoal.

If it be excluded from air and heated to whiteness in a tube of platina, exhausted after having been filled with hydrogen, it is found very little altered after the process. Its colour is a little darker, and it is rather denser: but no indications are given of any part of it having undergone fusion, volatilization, or decomposition. Before the process its specific gravity is such that it does not sink in sulphuric acid; but after, it rapidly falls to the bottom in this fluid.

The phenomena of its combustion are best witnessed in a retort filled with oxygen gas. When the bottom of the retort is gently heated by a spirit lamp, it throws off most vivid scintillations like those from the combustion of the bark of charcoal, and the mass burns with a brilliant light. A sublimate rises from it, which is boracic acid; and it becomes coated with a vitreous substance, which proves likewise to be boracic acid; and after this has been washed off, the residuum appears perfectly black, and requires a higher temperature for its inflammation than the olive-coloured substance; and by its inflammation produces a fresh portion of boracic acid.

In oxymuriatic acid gas, the peculiar inflammable substance occasions some beautiful phenomena. When this gas is brought in contact with it at common temperatures, it instantly takes fire and burns with a brilliant white light, a white substance coats the interior of the vessel in which the experiment is made, and the peculiar substance is found covered by a white film, which by washing affords boracic acid, and leaves a black matter, which is not spontaneously inflammable in a fresh portion of the gas; but which inflames in it by a gentle heat, and produces boracic acid.

The peculiar inflammable substance, when heated nearly to redness in hydrogen, or nitrogen, did not seem to dissolve in these gases, or to act upon them; it merely gained a darker shade of colour, and a little moisture rose from it, which condensed in the neck of the retort in which the experiment was made.

On the fluid menstrua containing oxygen, it produced effects which might be looked for from the phenomena of its agency on gases, When thrown into concentrated nitric acid, it rendered it bright red, so that nitrous gas was produced and absorbed, but it did not dissolve rapidly till the acid was heated; when there was a considerable effervescence, the peculiar substance disappeared, nitrous gas was evolved, and the fluid afforded boracic acid.

It did not act upon concentrated sulphuric acid till heat was applied; it then produced a slight effervescence; the acid became black at its points of contact with the solid; and a deep brown solution was formed, which, when neutralized by potash, gave a black precipitate.

When heated in a strong solution of muriatic acid, it gave it a faint tint of green; but there was no vividness of action, or considerable solution.

On acetic acid heated, it had no perceptible action.

It combined with the fixed alkalies, both by fusion and aqueous solution, and formed pale olive-coloured compounds, which gave dark precipitates when decomposed by muriatic acid.

When it was kept long in contact with sulphur in fusion, it slowly dissolved, and the sulphur acquired an olive tint. It was still less acted upon by phosphorus, and after an hour's exposure to it, had scarcely diminished in quantity, but the phosphorus had gained a tint of pale green.

It did not combine with mercury, when they were heated together.

These circumstances are sufficient to shew, that the combustible substance obtained from boracic acid by the agency of potassium, is different from any other known species of matter, and it seems, as far as the evidence extends, to be the same as that procured from it by electricity; and the two series of facts, seem fully to establish the decomposition, and recomposition of the acid.

From the large quantity of potassium required to decompose a small quantity of the acid, it is evident that the boracic acid must contain a considerable proportion of oxygen. I have endeavoured to determine the relative weights of the peculiar inflammable matter and oxygen, which compose a given weight of boracic acid; and to this end I made several analytical and synthetical experiments: I shall give the results of the two which I consider as most accurate.

Twenty grains of boracic acid and thirty grains of potassium, were made to act upon each other by heat in a tube of brass; the result did not effervesce when washed with diluted muriatic acid; and there were obtained after the process, by slight lixiviation in warm water, two grains and about $\frac{6}{10}$ of the olive-coloured matter. Now thirty grains of potassium, would require about five grains of oxygen, to form thirty-five of potash; and according to this estimation, boracic acid must consist of about one of the peculiar inflammable substance, to nearly two of oxygen.

A grain of the inflammable substance in very fine powder, and diffused over a large surface, was set fire to in a retort, containing twelve cubical inches of oxygen; three cubical inches of gas were absorbed, and the black residuum collected after the boracic acid had been dissolved, was found to equal five-eighths of a grain. This, by a second combustion, was almost entirely converted into boracic acid, with the absorption of two cubical inches and one-eighth more of oxygen. The thermometer in this experiment was at 58° Fahrenheit, and the barometer at 30·2.

According to this result, boracic acid would consist of one of the inflammable matter, to about 1·8 of oxygen; and the dark residual substance, supposing it to be simply the inflammable matter combined with less oxygen than is sufficient to constitute boracic acid, would be an oxide, consisting of about 4·7 of inflammable matter, to 1·55 of oxygen.

These estimations, I do not however venture to give, as entirely correct. In the analytical experiments, there are probably sources of error, from the solution of a part of the inflammable matter, and it possibly may retain alkali, which cannot be separated by the acid. In the synthetical process, in which washing is employed, and so small a quantity of matter used, the results are still less to be depended upon; they must be considered only as imperfect approximations.

From the general tenor of the facts, it appears that the combustible matter obtained from boracic acid, bears the same relation to that substance, as sulphur and phosphorus do to the sulphuric and phosphoric acids. But is it an elementary inflammable body, the pure basis of the acid? or is it not, like sulphur and phosphorus, compounded?

Without entering into any discussion concerning ultimate elementary matter, there are many circumstances which favour the idea, that the dark olive substance, is not a simple body; its being non-conducting, its change of colour by being heated in hydrogen gas, and its power of combining with the alkalies; for these properties in general belong to primary compounds, that are known to contain oxygen.

I heated the olive coloured substance with potassium, there was a combination, but without any luminous appearance, and a gray metallic mass was formed; but from the effect of this upon water, I could not affirm that any oxygen had been added to the metal, the gas given off had a peculiar smell, and took up more oxygen by detonation than pure hydrogen, from which it seems probable, that it held some of the combustible matter in solution.

It occurred to me, that if the pure inflammable basis were capable of being deoxigenated by potassium, it would probably possess a stronger affinity for oxygen than hydrogen, and therefore be again brought to its former state by water. I made another experiment on the operation of potassium, on the olive-coloured substance, and exposed the mixture to a small quantity of ether, hoping that this might contain only water enough to oxigenate the potassium; but the same result occurred as in the last case; and a combination of potash and the olive-coloured substance was produced, insoluble in ether.

I covered a small globule of potassium, with four or five times its weight of the olive-coloured matter, in a platina tube exhausted, after being filled with hydrogen; and heated the mixture to whiteness: no gas was evolved. When the tube was cooled, naphtha was poured into it, and the result examined under naphtha. Its colour was of a dense black. It had a lustre scarcely inferior to that of plumbago. It was a conductor of electricity. A portion of it thrown into water, occasioned a slight effervescence; and the solid matter separated, appeared dark olive, and the water became slightly alkaline. Another portion examined, after being exposed to air for a few minutes, had lost its conducting power, was brown on the surface, and no longer produced an effervescence in water.

Some of the olive inflammable matter, with a little potassium, was heated to whiteness, covered with iron filings, a dark metalline mass was formed, which conducted electricity, and which produced a very slight effervescence in water, and gave by solution in nitric acid, oxide of iron and boracic acid.

The substance which enters into alloy with potassium, and with iron, I am inclined to consider, as the true basis of the boracic acid.

In the olive-coloured matter, this basis seems to exist in union with a little oxygen; and when the olive-coloured substance is dried at common temperatures, it likewise contains water.

In the black non-conducting matter, produced in the combustion of the olive-coloured substance, the basis is evidently combined with much more oxygen, and in its full state of oxigenation, it constitutes boracic acid.

From the colour of the oxides, their solubility in alkalies, and from their general powers of combination, and from the conducting nature and lustre of the matter produced by the action of a small quantity of potassium upon the olive-coloured substance, and from all analogy; there is strong reason to consider the boracic basis as metallic in its nature, and I venture to propose for it the name of *boracium.**

VII. *Analytical Inquiries respecting Fluoric Acid.*

I have already laid before the Society, the account of my first experiments on the action of potassium, on fluoric acid gas.†

I stated, that the metal burns when heated in this elastic fluid, and that there is a great absorption of the gas.

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* [From his subsequent researches, the author arrived at the conclusion that the basis of this acid is more analogous to carbon, and he proposed for it, in accordance, the name boron: vide note p. 232, Vol. IV.]

† Page 113, note. The combustion of potassium in fluoric acid, I have since seen mentioned in the number of the Moniteur, already so often quoted, as observed by MM. Gay Lussac and Thenard; but no notice is taken of the results.

Since the time that this communication was made, I have carried on various processes, with the view of ascertaining, accurately, the products of combustion, and I shall now describe their results.

When fluoric acid gas, that has been procured in contact with glass, is introduced into a plate glass retort, exhausted after being filled with hydrogen gas, white fumes are immediately perceived. The metal loses its splendour, and becomes covered with a grayish crust.

When the bottom of the retort is gently heated, the fumes become more copious; they continue for some time to be emitted, but at last cease altogether.

If the gas is examined at this time, its volume is found to be a little increased, by the addition of a small quantity of hydrogen.

No new fumes are produced by a second application of a low heat; but when the temperature is raised nearly to the point of sublimation of potassium, the metal rises through the crust, becomes first of a copper colour and then of a bluish black, and soon after inflames and burns with a most brilliant red light.

After this combustion, either the whole or a part of the fluoric acid, according as the quantity of potassium is great or small, is found to be destroyed or absorbed. A mass of a chocolate colour remains in the bottom of the retort; and a sublimate, in some parts chocolate, and in others yellow, is found round the sides, and at the top of the retort.

When the residual gas afforded by this operation, is washed with water, and exposed to the action of an electrical spark mixed with oxygen gas, it detonates and affords a diminution, such as might be expected from hydrogen gas.

The proportional quantity of this elastic fluid differs a little in different operations. When the fluoric acid has not been artificially dried, it amounts to one-sixth or one-seventh of the volume of the acid gas used; but when the fluoric acid has been long exposed to calcined sulphate of soda, it seldom amounts to one-tenth.

I have endeavoured to collect large quantities of the chocolate-coloured substance for minute examination; but some difficulties occurred.

When I used from eighteen to twenty grains of potassium, in a retort containing from twenty to thirty cubical inches of fluoric acid gas, the intensity of the heat was such, as to fuse the bottom of the retort, and destroy the results.

In a very thick plate glass retort, containing about nineteen cubical inches of gas, I once succeeded in making a decisive experiment on ten grains and a half of potassium, and I found that about fourteen cubical inches of fluoric acid disappeared, and about two and a quarter of hydrogen gas were evolved. The barometer stood at 30·3, and the thermometer at 61° Fahrenheit; the gas had not been artificially dried. In this experiment there was very little sublimate; but the whole of the bottom of the retort was covered with a brown crust, and near the point of contact with the bottom, the substance was darker coloured, and approaching in its tint to black.

When the product was examined by a magnifier, it evidently appeared consisting of different kinds of matter; a blackish substance, a white, apparently saline substance, and a substance having different shades of brown and fawn colour.

The mass did not conduct electricity, and none of its parts could be separated, so as to be examined as to this property.

When a portion of it was thrown into water, it effervesced violently, and the gas evolved had some resemblance in smell to phosphuretted hydrogen, and was inflammable.

When a part of the mass was heated in contact with air, it burnt slowly, lost its brown colour, and became a white saline mass.

When heated in oxygen gas, in a retort of plate glass, it absorbed a portion of oxygen, but burnt with difficulty, and required to be heated nearly to redness; and the light given out was similar to that produced by the combustion of liver of sulphur.

The water which had acted upon a portion of it was examined; a number of chocolate-coloured particles floated in it. When the solid matter was separated by the filter, the fluid was found to contain fluate of potash, and potash. The solid residuum was heated in a small glass retort in oxygen gas; it burnt before it had attained a red heat, and became white. In this process, oxygen was absorbed, and acid matter produced. The remainder possessed the properties of the substance formed from fluoric acid gas holding siliceous earth in solution, by the action of water.

In experiments made upon the combustion of quantities of potassium equal to from six to eleven grains, the portion of matter separable from the water has amounted to a very small part of a grain only, and operating upon so minute a scale, I have not been able to gain fully decided evidence, that the inflammable part of it is the pure basis of the fluoric acid; but with respect to the decomposition of this body by potassium, and the existence of its basis at least combined with a smaller proportion of oxygen in the solid product generated, and the regeneration of the acid by the ignition of this product in oxygen gas, it is scarcely possible to entertain a doubt.

The decomposition of the fluoric acid by potassium, seems analogous to that of the acids of sulphur and phosphorus. In neither of these cases are the pure bases, or even the bases in their common form evolved; but new compounds result, and in one case sulphurets, and sulphites, and in the other phosphurets, and phosphites of potash, are generated.

As silex was always obtained during the combustion of the chocolate-coloured substance obtained by lixiviation, it occurred to me that this matter might be a result of the operation, and that the chocolate substance might be a compound of the siliceous and fluoric bases in a low state of oxigenation, with potash; and this idea is favoured by some trials that I made to separate silex from the mass, by boiling it in concentrated fluoric acid; the substance did not seem to be much altered by this process, and still gave silex by combustion.

I endeavoured to decompose fluoric acid gas in a perfectly dry state, and which contained no siliceous earth; and for this purpose I made a mixture of one hundred grains of dry boracic acid, and two hundred grains of fluor spar, and placed them in the bottom of an iron tube, having a stop-cock and a tube of safety attached to it.

The tube was inserted horizontally in a forge, and twenty grains of potassium, in a proper iron tray, introduced into that part of it where the heat was only suffered to rise to dull redness. The bottom of the tube was heated to whiteness, and the acid acted upon by the heated potassium, as it was generated. After the process was finished, the result in the tray was examined.

It was in some parts black, and in others of a dark brown. It did not effervesce with water: and when lixiviated, afforded a dark brown combustible mass, which did not conduct electricity, and which when burnt in oxygen gas, afforded boracic, and fluoric acid. It dissolved with violent effervescence in nitric acid; but did not inflame spontaneously in oxymuriatic acid gas.

I have not as yet examined any of the other properties of this substance; but I am inclined to consider it as a compound of the olive-coloured oxide of boracium, and an oxide of the fluoric basis.

In examining the dry fluoric acid gas, procured in a process similar to that which has been just described, it gave very evident marks of the presence of boracic acid.

As the chocolate-coloured substance is permanent in water, it occurred to me that it might possibly be producible from concentrated liquid fluoric acid at the negative surface in the Voltaic circuit.

I made the experiment with platina surfaces, from a battery of two hundred and fifty plates of six inches, on fluoric acid the densest that could be obtained by the distillation of fluor spar and concentrated sulphuric acid of commerce, in vessels of lead. Oxygen and hydrogen were evolved, and a dark brown matter separated at the deoxidating surface; but the result of an operation conducted for many hours, merely enabled me to ascertain that it was combustible, and produced acid matter in combustion; but I cannot venture to draw the conclusion that this acid was fluoric acid, as it was not impossible that some sulphurous, or sulphuric acid might likewise exist in the solution.

I heated the olive-coloured inflammable substance, obtained from the boracic acid, in common fluoric acid gas in a plate glass retort; the temperature was raised till the glass began to fuse; but no change, indicating a decomposition, took place.

I heated six grains of potassium with four grains of powdered fluor spar in a green glass tube filled with hydrogen; there was a slight ignition, a minute quantity of hydrogen gas was evolved, and a dark gray mass was produced, which acted upon water with much effervescence, but left no solid inflammable residuum.*

VIII. *Analytical Experiments on Muriatic Acid.*

I have made a greater number of experiments upon this substance, than upon any of the other subjects of research that have been mentioned; it will be impossible to give any more than a general view of them within the limits of the Bakerian Lecture.

Researches carried on some years ago, and which are detailed in the Journals of the Royal Institution, shewed that there were little hopes of decomposing muriatic acid, in its common form, by Voltaic electricity. When aqueous solution of muriatic acid is acted upon, the water alone is decomposed; and the Voltaic electrization of the gas affords no indications of its decomposition; and merely seems to shew, that this elastic fluid contains much more water than has been usually suspected.

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* [Vide the Author's later researches on the fluoric combinations: at this time, the chemical history of them was very imperfect,—as is indicated by the terms applied to them;—it was not then known that silica, or its basis, is an essential ingredient of the gas which in the text is named fluoric acid gas; nor was the existence of fluo-boracic acid gas, as a distinct compound, then ascertained.]

I have already laid before the Society an account of some experiments made on the action of potassium on muriatic acid. I have since carried on the same processes on a larger scale, but with precisely similar results.

When potassium is introduced into muriatic acid gas, procured from muriate of ammonia and concentrated sulphuric acid, and freed from as much moisture as muriate of lime is capable of attracting from it, it immediately becomes covered with a white crust, it heats spontaneously, and, by the assistance of a lamp, acquires in some parts the temperature of ignition, but does not inflame. When the potassium and the gas are in proper proportions, they both entirely disappear; a white salt is formed, and a quantity of pure hydrogen gas evolved, which equals about one-third of the original volume of the gas.

By eight grains of potassium employed in this way, I effected the absorption of nearly twenty-two cubical inches of muriatic acid gas; and the quantity of hydrogen gas produced was equal to more than eight cubical inches.

The correspondence between the quantity of hydrogen generated in cases of this kind, and by the action of potassium upon water, combined with the effects of ignited charcoal upon muriatic acid gas, by which a quantity of inflammable gas is produced equal to more than one-third of its volume, seemed to shew, that the phenomena merely depended upon moisture combined with the muriatic acid gas.*

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* When the Voltaic spark is taken continuously, by means of points of charcoal in muriatic acid gas over mercury, muriate of mercury is

To determine this point with more certainty, however, and to ascertain whether or not the appearance of the hydrogen was wholly unconnected with the decomposition of the acid, I made two comparative experiments on the quantity of muriate of silver, furnished by two equal quantities of muriatic acid, one of which had been converted into muriate of potash by the action of potassium, and the other of which had been absorbed by water; every care was taken to avoid sources of error; and it was found that there was no notable difference in the weight of the results.

There was no proof then, that the muriatic acid had been decompounded in these experiments; and there was every reason to consider it as containing in its common aëriform state, at least one-third of its weight of water; and this conclusion we shall find warranted by facts, which are immediately to follow.

I now made a number of experiments, with the hopes of obtaining the muriatic acid free from water.

I first heated to whiteness, in a well luted porcelain retort, a mixture of dry sulphate of iron, and muriate

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rapidly formed, a volume of inflammable gas, equal to one-third of the original volume of the muriatic acid gas, appears. The acid gas enters into combination with the oxide of mercury, so that water enough is present in the experiment to form oxide sufficient to absorb the whole of the acid.

[The attention of the student of chemistry cannot be too carefully given to the progress of this inquiry into the nature of muriatic acid, which terminated in the subversion of the old doctrine, and the establishment of the doctrine now universally admitted, that muriatic acid gas contains no water; but is a compound of chlorine or oxymuriatic gas and hydrogen, and that oxymuriatic gas itself is a simple, |*i. e.* an hitherto undecompounded substance. This remark I have thought it right to make on the opening of the inquiry, which from this time proceeds step by step, from experiments to doubt, and from doubt to experiments, until all doubts are removed, and demonstration is arrived at.]

of lime which had been previously ignited; but a few cubic inches of gas only were obtained, though the mixture was in the quantity of several ounces; and this gas contained sulphurous acid. I heated dry muriate of lime, mixed both with phosphoric glass and dry boracic acid, in tubes of porcelain and of iron, and employed the blast of an excellent forge; but by neither of these methods was any gas obtained, though when a little moisture was added to the mixtures, muriatic acid was developed in such quantities, as almost to produce explosions.

The fuming muriate of tin, *the liquor of Libavius*, is known to contain dry muriatic acid. I attempted to separate the acid from this substance, by distilling it with sulphur and with phosphorus; but without success. I obtained only triple compounds, in physical characters, something like the solutions of phosphorus, and sulphur in oil, which were non-conductors of electricity, which did not redden dry litmus paper, and which evolved muriatic acid gas with great violence, heat, and ebullition on the contact of water.

I distilled mixtures of corrosive sublimate and sulphur and of calomel and sulphur: when these were used in their common states, muriatic gas was evolved; but when they were dried by a gentle heat, the quantity was exceedingly diminished, and the little gas that was generated gave hydrogen by the action of potassium. During the distillation of corrosive sublimate and sulphur, a very small quantity of a limpid fluid passed over. When examined by transmitted light, it appeared yellowish green. It emitted fumes of muriatic acid, did not redden dry litmus paper, and deposited sulphur by the action of water. I am inclined to consider it as a modification of the substance discovered by Dr. Thomson, in his experiments on the action of oxymuriatic acid on sulphur.

MM. Gay Lussac and Thenard* have mentioned, that they endeavoured to procure dry muriatic acid by distilling a mixture of calomel and phosphorus, and that they obtained a fluid which they consider as a compound of muriatic acid, phosphorus, and oxygen. In distilling corrosive sublimate with phosphorus, I had a similar result, and I obtained the substance in much larger quantities, than by the distillation of phosphorus with calomel.

As oxymuriatic acid is slightly soluble in water, there was reason to suppose, reciprocally, that water must be slightly soluble in this gas; I endeavoured therefore to procure dry muriatic acid, by absorbing the oxygen from oxymuriatic acid gas by substances, which when oxygenated, produce compounds possessing a strong affinity for water. Phosphorus, it is well known, burns in oxymuriatic acid gas; though the results of this combustion, I believe, have never been minutely examined. With the hopes of procuring muriatic acid gas, free from moisture, I made the experiment. I introduced phosphorus into a receiver having a stop-cock, which had been exhausted, and admitted oxymuriatic acid gas. As soon as the retort was full, the phosphorus entered into combustion, throwing forth pale white flames. A white sublimate collected in the top of the retort, and a fluid as limpid as water, trickled down the sides of the neck. The gas seemed to be entirely absorbed, for when the stop-cock was opened, a fresh quantity of oxymuriatic acid, nearly as much as would have filled the retort, entered.

The same phenomenon of inflammation again took place, with similar results. Oxymuriatic acid gas was admitted till the whole of the phosphorus was consumed.

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* The Moniteur before quoted.

Minute experiments proved, that no gaseous muriatic acid had been evolved in this operation, and the muriatic acid was consequently to be looked for either in the white sublimate, or in the fluid which had formed in the neck of the retort.

The sublimate was in large portions, the fluid only in the quantity of a few drops. I collected, by different processes, sufficient of both for examination.

The sublimate emitted fumes of muriatic acid when exposed to air. When brought in contact with water, it evolved muriatic acid gas, and left phosphoric acid, and muriatic acid, dissolved in the water. It was a non-conductor of electricity, and did not burn when heated; but sublimed when its temperature was about that of boiling water, leaving not the slightest residuum. I am inclined to regard it as a combination of phosphoric, and muriatic acid in their dry states.

The fluid was of a pale greenish yellow tint, and very limpid; when exposed to air, it rapidly disappeared, emitting dense white fumes which had a strong smell differing a little from that of muriatic acid.

It reddened litmus paper in its common state, but had no effect upon litmus paper which had been well dried, and which was immediately dipped into it. It was a non-conductor of electricity. It heated when mixed with water, and evolved muriatic acid gas. I consider it as a compound of phosphorous acid, and muriatic acid, both free from water.*

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* I attempted to obtain dry muriatic acid likewise from the phosphuretted muriatic acid of MM. Gay Lussac and Thenard, by distilling it in retorts containing oxygen gas, and oxymuriatic acid gas. In the first

Having failed in obtaining uncombined muriatic acid in this way, I performed a similar process with sulphur, but I was unable to cause it to inflame in oxymuriatic acid gas. When it was heated in it, it produced an orange coloured liquid, and yellow fumes passed into the neck of the retort, which condensed into a greenish yellow fluid. By repeatedly passing oxymuriatic acid through this fluid, and distilling it several times in the gas, I rendered it of a bright olive colour, and in this case it seemed to be a compound of dry sulphuric, and muriatic acid, holding in solution a very little sulphur. When it was heated in contact with sulphur, it rapidly dissolved it, and then became of a bright red colour, and when saturated with sulphur, of a pale golden colour.* No permanent aëriform fluid was evolved in any of these operations, and no muriatic gas appeared, unless moisture was introduced.

As there seemed little chance of procuring uncombined muriatic acid, it was desirable to ascertain what would be the effects of potassium upon it in these singular compounds.

When potassium was introduced into the fluid, generated by the action of phosphorus on corrosive sublimate, at first it slightly effervesced, from the action of the liquid on the moist crust of potash surrounding it; but the metal soon appeared perfectly splendid, and swimming on the surface. I attempted to fuse it by heating the fluid, but it entered into ebullition at a

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case, the retort was shattered by the combustion of the phosphorus, with a violent explosion. In the second, compounds, similar to those described above, were formed.

* All these substances seem to be of the same nature as the singular compound, the sulphuretted muriatic acid, discovered by Dr. Thomson, noticed in page 194.

temperature below that of the fusion of the potassium; indeed the mere heat of the hand was sufficient for the effect. On examining the potassium, I found that it was combined at the surface with phosphorus, and gave phosphuretted hydrogen by its operation upon water.

I endeavoured, by repeatedly distilling the fluid from potassium in a close vessel, to free it from phosphorus, and in this way I succeeded in depriving it of a considerable quantity of this substance.

I introduced ten or twelve drops of the liquid, which had been thus treated, into a small plate glass retort, containing six grains of potassium; the retort was exhausted after having been twice filled with hydrogen, the liquid was made to boil, and the retort kept warm till the whole had disappeared as elastic vapour. The potassium was then heated by the point of a spirit lamp; it had scarcely melted, when it burst into a most brilliant flame, as splendid as that of phosphorus in oxygen gas, and the retort was destroyed by the rapidity of combustion.

In other trials made upon smaller quantities after various failures, I was at last able to obtain the results; there was no proof of the evolution of any permanent elastic fluid during the operation. A solid mass remained of a greenish colour at the surface, but dark gray in the interior. It was extremely inflammable, and often burnt spontaneously when exposed to air: when thrown upon water, it produced a violent explosion, with a smell like that of phosphuretted hydrogen. In the residuum of its combustion there was found muriate of potash, and phosphate of potash.

I endeavoured to perform this experiment in an iron tube, hoping that if the muriatic acid was decomposed in the process, its inflammable element, potassium and phosphorus, might be separated from each other by a high degree of heat; but in the first part of the operation the action was so intense, as to produce a destruction of the apparatus, and the stop-cock was separated from the tube with a loud detonation.

I heated potassium in the vapour of the compound of muriatic and phosphoric acid; but in this case, the inflammation was still more intense, and in all the experiments that I have hitherto tried, the glass vessels have been either fused or broken; the solid residuum has however appeared to be of the same kind as that I have just described.

The results of the operation of the sulphuretted compounds containing muriatic acid free from water upon potassium, are still more extraordinary than those of the phosphuretted compounds.

When a piece of potassium is introduced into the substance that distils over during the action of heated sulphur upon oxymuriatic acid, it at first produces a slight effervescence, and if the volume of the potassium considerably exceeds that of the liquid, it soon explodes with a violent report, and a most intense light.

I have endeavoured to collect the results of this operation, by causing the explosion to take place in large exhausted plate glass retorts; but, except in a case in which I used only about a quarter of a grain, I never succeeded. Generally the retort, though connected with the air-pump at the time, was broken into atoms; and the explosion produced by a grain of potassium, and an equal quantity of the fluid, has appeared to me considerably louder than that of a musket.

In the case in which I succeeded in exploding a quarter of a grain, it was not possible for me to ascertain if any gaseous matter was evolved; but a solid compound was formed of a very deep gray tint, which burnt, throwing off bright scintillations, when gently heated, which inflamed when touched with water, and gave most brilliant sparks, like those thrown off by iron in oxygen gas.

Its properties certainly differed from those of any compound of sulphur and potassium that I have seen: whether it contains the muriatic basis must however be still a matter of inquiry.

There is, however, much reason for supposing, that in the singular phenomena of inflammation and detonation that have been described, the muriatic acid cannot be entirely passive: and it does not seem unfair to infer, that the transfer of its oxygen and the production of a novel substance, are connected with such effects, and that the highly inflammable nature of the new compounds, partly depends upon this circumstance. I am still pursuing the inquiry, and I shall not fail immediately to communicate to the Society such results as may appear to me worthy of their attention.

IX. *Some general Observations, with Experiments.*

An experiment has been lately published, which appeared so immediately connected with the discussion entered into in the second section of this Paper, that I repeated it with much earnestness.

In Mr. Nicholson's Journal for December, Dr. Woodhouse has given an account of a process, in which the action of water caused the inflammation of a mixture of four parts of charcoal and one of pearlash that had been strongly ignited together, and the emission of ammonia from them. I thought it possible, that in this case a substance might be formed similar to the residuum described in page 151; but by cooling the mixture out of the contact of nitrogen, I found that no ammonia was formed; and this substance evidently owed its existence to the absorption of atmospherical air by the charcoal.*

The experiments that I have detailed on the acids offer some new views with respect to the nature of acidity. That a compound of muriatic acid with oxide of tin or phosphorus should not redden vegetable blues, might be ascribed to a species of neutralization, by the oxide or inflammable body; but the same reasoning will not apply to the dry compounds which contain acid matter only, and which are precisely similar as to this quality. Let a piece of dry and warm litmus paper be moistened with the compound of muriatic and phosphorous acid, it perfectly retains its colour. Let it then be placed upon a piece of moistened litmus paper, it instantly becomes of a bright red, heats and develops muriatic acid gas.

All the fluid acids that contain water are excellent conductors of electricity, in the class called that of imperfect conductors; but the compounds to which I have just alluded, are non-conductors in the same degree as oils, with which they are perfectly miscible. When I first examined muriatic acid, in its combinations free from moisture, I had great hopes of decomposing them by electricity; but there was no action without contact of the wires, and the spark seemed to separate no one of their constituents, but only to render them gaseous. The circumstance likewise applies to the boracic acid, which is a good conductor as long as it contains water; but which, when freed from water and made fluid by heat, is then a non-conductor.

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* Potash or pearlash is easily decomposed by the combined attractions of charcoal and iron; but it is not decomposable by charcoal, or, when perfectly dry, by iron alone. Two combustible bodies seem to be required by their combined affinities for the effect; thus in the experiment with the gun-barrel, iron and hydrogen are concerned. I consider Homberg's pyrophorus as a triple compound of potassium, sulphur, and charcoal; and in this ancient process, the potash is probably decomposed by two affinities. The substance is perfectly imitated by heating together ten parts of charcoal, two of potassium, and one of sulphur.

When I first shewed the production of potassium to Dr. Wollaston in October 1807, he stated, that this new fact induced him to conceive that the action of potash upon platina, was owing to the formation of potassium, and proposed it, as a matter of research, whether the alkali might not be decomposed by the joint action of platina and charcoal.

The alkalies and the earthy compounds, and the oxides, as dry as we can obtain them, though non-conductors when solid, are, on the contrary, all conductors when rendered fluid by heat.

When muriatic acid, existing in combination with phosphorous or phosphoric acid, is rendered gaseous by the action of water, the quantity of this fluid that disappears, at least equals from one-third to two-fifths of the weight of the acid gas produced; a circumstance that agrees with the indications given by the action of potassium.*

I attempted to procure a compound of dry muriatic and carbonic acids, hoping that it might be gaseous, and that the two acids might be decomposable at the same time by potassium. The process that I employed was by passing corrosive sublimate in vapour through charcoal ignited to whiteness; but I obtained a very small quantity of gas, which seemed to be a mixture of common muriatic acid gas and carbonic acid gas; a very minute portion of running mercury only was obtained, by a long continuation of the process; and the slight decomposition that did take place, I am inclined to attribute to the production of water, by the action of the hydrogen of the charcoal upon the oxygen of the oxide of mercury.*

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* Page 199.

In mixing muriatic acid gas with carbonic acid, or oxygen, or hydrogen, the gases being in their common states, as to moisture, there was always a cloudiness produced; doubtless owing to the attraction of their water to form liquid muriatic acid.

On fluoric acid gas no such effect was occasioned. This fact, at first view, might be supposed to shew, that the hydrogen evolved by the action of potassium upon fluoric acid gas, is owing to water in actual combination with it, like that in muriatic acid gas, and which may be essential to its elastic state; but it is more probable, from the smallness of the quantity, and from the difference of the quantity in different cases, that the moisture is merely in that state of diffusion or solution in which it exists in gases in general, though from the disposition of water to be deposited in this acid gas in the form of an acid solution, it must be either less in quantity, or in a less free state, so as to require for its exhibition much more delicate hygrometrical tests.

The facts advanced in this Lecture, afford no new arguments in favour of an idea to which I referred in my last communication to the Society, that of hydrogen being a common principle in all inflammable bodies; and except in instances which are still under investigation, and concerning which no precise conclusions can as yet be drawn, the generalization of Lavoisier happily applies to the explanation of all the new phenomena.

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* These facts and the other facts of the same kind, explain the difficulty of the decomposition of the metallic muriates in common processes of metallurgy. They likewise explain other phenomena in the agencies of muriatic salts. In all cases when a muriatic salt is decomposed by an acid, and muriatic acid gas set free, there appears to be a double affinity, that of the acid for the basis, and of the muriatic acid for water; pure muriatic acid does not seem capable of being displaced by any other acid.

In proportion as progress is made towards the knowledge of pure combustible bases, so in proportion is the number of metallic substances increased; and it is probable that sulphur and phosphorus, could they be perfectly deprived of oxygen, would belong to this class of bodies. Possibly their pure elementary matter may be procured by distillation, at a high heat, from metallic alloys, in which they have been acted upon by sodium or potassium. I hope soon to be able to try this experiment.

As our inquiries at present stand, the great general division of natural bodies is into matter which is, or may be supposed to be, metallic, and oxygen; but till the problem concerning the nature of nitrogen is fully solved, all systematic arrangements made upon this idea must be regarded as premature.

EXPLANATION OF THE FIGURES.

Fig. 1. The retort of plate glass for heating potassium in gases.

Fig. 2. The tray of platina for receiving the potassium.

Fig. 3. The platina tube for receiving the tray in experiments of distillation.

Fig. 4. The apparatus for taking the Voltaic spark in sulphur and phosphorus.

V.

NEW ANALYTICAL RESEARCHES ON THE NATURE OF CERTAIN BODIES, BEING AN APPENDIX TO THE BAKERIAN LECTURE FOR 1808.*

I. *Further Inquiries on the Action of Potassium on Ammonia*† *and on the Analysis of Ammonia.*

THE most remarkable circumstances occurring in the action of potassium upon ammonia are the disappearance of a certain quantity of nitrogen, and the conversion of a part of the potassium into potash.

The first query which I advanced in the last Bakerian Lecture, on this obscure and difficult subject, was whether the gas developed in the first part of the process of the absorption of ammonia by potassium is hydrogen, or a new species of inflammable aëriform substance, the basis of nitrogen?

Experiments made to determine this point have proved, as I expected, that the gas differs in no respect from that given out during the solution of zinc in sulphuric acid; or that produced during the action of potassium on water. By slow combustion with oxygen, it generates pure water only, and its weight, in a case in which it was mixed with atmospherical air, precisely corresponded with that of an equal quantity of hydrogen.

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* [From the Phil. Trans. for 1809.]

† The accounts of the principal facts respecting the action of potassium on ammonia, in this communication, were read before the Royal Society, February 2, 1809. The paper was ordered to be printed March 16, 1809. At that time, having stated to the Council that I had since made some new experiments on this matter, and on the subjects discussed in the Bakerian Lecture for 1808, I received permission to add them to the detail of the former observations for publication.

Another query which I put is, has nitrogen a metallic basis which alloys with the metals employed in the experiment?

This query I cannot answer in so distinct a manner; but such results as I have been able to obtain are negative.

I have examined the potassium generated in the process. It has precisely the same properties as potassium produced in the common experiment of the gun-barrel; and gives the same results by combustion in oxygen, and by the action of water.

In cases in which I had distilled the olive-coloured fusible substance in an iron tray, the surface of the tray appeared much corroded, the metal was brittle, and appeared crystallized. I made a solution of it in muriatic acid; but hydrogen alone was evolved.

I distilled a quantity of the fusible substance from 9 grains of potassium in an iron vessel, which communicated with a receiver containing about 100 grains of mercury, and by a narrow glass tube the gas generated was made to pass through the mercury; the object of this process was to detect if any of the same substance, as that existing in the amalgam from ammonia, was formed; but during the whole period of distillation, the mercury remained unaltered, in its appearance, and did not effervesce in the slightest degree when thrown into water.

That the nitrogen which disappears in this experiment is absolutely converted into oxygen and hydrogen, and that its elements are capable of being furnished from water, is a conclusion of such importance, and so unsupported by the general order of chemical facts, that it ought not to be admitted, except upon the most rigid and evident experimental proofs.

I have repeated the experiment of the absorption of ammonia by potassium in trays of platina or iron, and its distillation in tubes of iron more than twenty times, and often in the presence of some of the most distinguished chemists in this country, from whose acuteness of observation, I hoped no source of error could escape.

The results, though not perfectly uniform, have all been of the same kind as those described in page 156. Six grains of potassium, the quantity constantly used, always caused the disappearance of from 10 to 12·5 cubical inches of well dried ammonia. From 5·5 to 6 cubical inches of hydrogen were produced, a quantity always inferior to that evolved by the action of an equal portion of the metal upon water. In the distillation from 11 to 17 cubical inches of elastic fluid were evolved, and from 1·5 to 2·5 grains of potassium regenerated.

The quantity of ammonia in the products, varied from a portion that was scarcely perceptible to one-twelfth or one-thirteenth of the whole volume of elastic fluid: and it was least in those cases in which the absence of moisture was most perfectly guarded against. Under these circumstances likewise, more potassium was revived; and the unabsorbable elastic fluid, and particularly the hydrogen in smaller proportion.

When the products of distillation were collected at different periods, it was uniformly found that the proportion of nitrogen to the hydrogen diminished as the process advanced.

The first portions contained considerably more nitrogen in proportion, than the gases evolved during the electrization of ammonia, and the last portions less.

I shall give the results of an experiment, in which the gases produced in distillation were collected in four different vessels, and in which every precaution was taken to avoid sources of inaccuracy.

The barometer was at 29·8, thermometer 65° Fahrenheit.

6 grains of potassium absorbed 12 cubic inches of well dried ammonia. The metal was heated in a tray of platina, and the gas contained in a retort of plate glass.

5·8 cubical inches of hydrogen were produced.

The fusible substance was distilled in an iron tube of the capacity of 3 cubical inches and half filled with hydrogen, the adaptors connected with the mercurial apparatus contained ·8 of common air.

The first portion of gas collected (the heat being very slowly raised, and long before it had rendered the vessel red), equalled 7·5 cubical inches. It contained ·6 of ammonia. 7 of the residuum detonated with 4½ of oxygen gas left a residuum of 4.

The second portion, equal to 3 cubical inches, contained no ammonia. 7·2 measures of it, detonated with 3·8 of oxygen, left a residuum of 3·5.

The third portion was equal to 5 cubical inches; at this time the tube was white hot; it contained no ammonia. 8·5 of it detonated with 4·5 of oxygen diminished to 2·5.

The last portion was a cubical inch and a half, collected when the heat was most intense. 4·5 measures, with 3·75 of oxygen, left a residuum of 2·8.

The iron tube contained, after the experiment, (as was ascertained by admitting hydrogen when it was cool), 2·7 of gas; which seemed of the same composition as the last portion. The adaptors must have contained ·8 of a similar gas.

The tube contained potash in its lowest part, and in its upper part potassium, which gave by its action upon water 1¾ cubical inch of hydrogen.

As the largest quantity of hydrogen is always produced at that period of the process, in which the potassium must be conceived to be regenerated, and in which the gases being in the nascent state, its power of action upon them would be greatest, it occurred to me, that if nitrogen was decomposed in the operation, there would probably be a larger quantity of it destroyed by the distillation of the fusible substance, with a fresh quantity of potassium, than by the distillation of it in its common state. On this idea I made several experiments; the results did not differ much from each other, and were such as I had expected. I shall describe one process made with the same apparatus as that which I have just detailed.—Barometer was at 29·5, thermometer 70° Fahrenheit.

6 grains of potassium were employed in an iron tray; 10 cubical inches of ammonia were absorbed, a small globule of metal remained unconverted into the fusible substance. A fresh piece of potassium, weighing six grains, was introduced into the tray.

The iron tube and the adaptors (having together a capacity equal to 4·3 cubical inches) contained common air.

The gas was collected in three portions, there was no absorbable quantity of ammonia in either of them.

The first portion, that produced before the tube became red, was eight cubical inches. 10·25 of it, detonated with 3·5 of oxygen, diminished to 8.

The second portion equalled five cubical inches; 9⅓ of this, with 5 oxygen, left a residuum of 3¾.

Of the third portion, 2 cubical inches and ⅓ came over. 9 of it, detonated with 5 of oxygen gas, left a residuum of 1·4.

The iron tube and the adaptors contained, at the end of the experiment, as was proved by cooling and the admission of hydrogen 2·3 cubical inches of gas, which appeared of the same composition as the third portion. Nearly 7 grains of potassium were recovered.

A comparison of these results, with those stated in the preceding page, will fully prove, that there is a much smaller proportion of nitrogen to the hydrogen, in the case in which the olive-coloured substance is distilled with potassium, than in the other case, and there is likewise a larger quantity of potassium converted into potash.

The loss of nitrogen, and the addition of oxygen to the potassium, are sufficiently distinct in both processes; and the want of a correspondence between these results, and those of the experiment detailed, are not greater than might be expected, when all the circumstances of the operation are considered. In the instance, in which a double quantity of potassium was employed, more potash must have been formed from the oxygen of the common air in the tubes; and the fusible substance, in passing through the atmosphere, absorbs in different cases different quantities of oxygen and of moisture; during the intervals of the removal of the different portions of gas likewise, some globules are lost.

In instances when the heat has been more rapidly raised, I have generally found more potassium destroyed, and less nitrogen in proportion in the aëriform products. In such cases likewise, the loss of weight has been much greater; the gases have been always clouded, and the adaptors, after being exposed to a moist air, emitted a smell of ammonia; from which it seems likely that small quantities of the dark gray substance described, are sometimes carried over undecompounded in the operation.

In some late experiments, I substituted for the iron tube, a tube of copper, which had been bored from a solid piece, and the sides of which were nearly a quarter of an inch in thickness. My object in using this tube was not only to prevent the heat from being too rapidly communicated to the fusible substance, but likewise to be secure that no metallic oxide was present, for though the iron tubes had been carefully cleaned, yet still it was possible that some oxide, which could not be separated from the welded parts, might exist, which of course would occasion the disappearance of a certain quantity of potassium.

I shall give the results of one of the processes, which I regard as most correct, made in the tube of copper. The barometer was at 30·5; thermometer was at 59° Fahrenheit.

The tube contained two cubical inches and half, and was filled with hydrogen.

6 grains of potassium, which had absorbed 13 cubical inches of ammonia in a copper tray were employed.

The adaptors connected with the mercurial apparatus and the stop-cocks, contained ·7 of atmospherical air.

The gas given off was collected in two portions.

The first portion was equal to 11 cubical inches. It contained ·8 of ammonia, 11 of the residuum, detonated with 8 of oxygen, left 8.

The second portion equalled 2 cubical inches. They contained no ammonia. 10 of this gas, with 8 of oxygen detonated, left a residuum of 10.

There remained in the tube and adaptors 1·1 cubical inch of gas.

The quantity of hydrogen produced by the action of the potassium, which had been regenerated, equalled 4·5 cubical inches.

In this experiment the heat was applied much more slowly than in any of those in which the iron tube was used, and even at the end of the operation, the temperature was little more than that of cherry red.

In the upper part of the stop-cock there was found a minute quantity of gray powder, which gave ammonia by the operation of moisture.

In no case, in which I have used the copper tube in like processes of slow distillation, has there been less than 4 grains of potassium revived; and the proportion of nitrogen to the hydrogen in the gas evolved has been uniformly much greater than in processes of rapid distillation in the tubes of iron; but the whole quantity of elastic matter procured considerably less.

Copper has a much stronger affinity for potassium* than iron. It occurred to me as probable, that this attraction, by preventing the potassium from rising in vapour at its usual temperature, and likewise by the general tendency of such combination to give greater density, might occasion a diminution of its action upon the nitrogen in the nascent state. Ammonia has a strong attraction for the oxide of copper, and it consequently is not unlikely that the fusible substance may combine with metallic copper, and that this compound may not be entirely destroyed in the distillation. And assuming this, it may be conceived that the loss of hydrogen partly depends upon some combination of the basis of ammonia with copper.

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* Copper heated in potassium speedily dissolves, and diminishes its fusibility; but potassium requires a white heat to enable it to combine with iron. In another experiment, in which I distilled the fusible substance in an iron tray, contained in the copper tube, a considerable quantity of copper, that had been dissolved, was found in the state of powder deposited upon the tray, or loose in the bottom of the tube.

I had a tube, of the capacity of 2¼ cubical inches, made of wrought platina, cemented by means of fine gold solder. The fusible substance was obtained (as usual from six grains of potassium) in a tray of platina, where it was brought in contact with a large surface of platina wire; the distillation was slowly conducted; but before the temperature of the tube had approached to that of ignition, it dissolved and gave way at the points where it was soldered, and a violent combustion took place. Only 7 cubical inches of gas were collected; but of this, allowing for the hydrogen that filled the tube, nearly ⅕ were nitrogen.

I am making preparations for performing the experiment in a bored tube made from a single piece of platina, and likewise in tubes made of other metals, and I hope to be able, in a short time, to have the honour of laying the results before the Society.*

I shall make no apology for bringing forward the investigation in its present imperfect state, except by stating that my motive for so doing, is the desire of being assisted or corrected by the opinions and advice of the learned chemical philosophers belonging to this illustrious body. In an investigation connected with almost all the theoretical arrangements of chemistry, and in operations of so much delicacy, it will, I conceive, be allowed, that it is scarcely possible to proceed with too much caution, or to multiply facts to too great an extent.

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* [Vide Sections ii. & iii. of the following paper for the results.]

The different phenomena presented by the processes of distillation in different metallic tubes, may lead to new explanations of this intricate subject, and though the facts cannot be easily accounted for, except on the supposition that nitrogen is an oxide, yet till the proportions and weights are distinctly ascertained, the inquiry cannot be considered as far advanced, for in an experiment, in which the processes are so complicated and delicate, and in which the data are so numerous, it is not easy to be satisfied that every source of error has been avoided, and that every circumstance had been examined and reasoned upon.

All conclusions on the action of potassium on ammonia, are immediately dependent upon the results of the electrical analysis of the volatile alkali. In a letter, which I received in the course of the last month from Dr. Henry, that excellent chemist has stated that he conceives I have underrated the quantity of nitrogen in ammonia, according to the proportions given in the Bakerian Lecture for 1807. This notice has induced me to repeat the experiment, under new circumstances, and I find not the slightest reason for doubting of the entire accuracy of my former results.

In the new trial, I used mercury which had been recently boiled in the tube for electrization; the ammonia was introduced after being long dried by caustic potash, from a receiver in which it had not been generated, and which had likewise been inverted over boiling mercury. The gas left no perceptible residuum, when absorbed by water deprived of air by boiling. In this process, 15 measures of ammonia expanded, so as to fill 27 measures; and the hydrogen by detonation with oxygen, over water freed as much as possible from air, proved to be to the nitrogen as 73·8 to 26·2. In the experiment three explosions were made, the oxygen being deficient in the first two; so that no nitrogen could have been condensed in the form of nitric acid.

Except when precautions of this kind are employed, as I have before noticed, no accurate data can be obtained respecting the proportions of permanent gases obtained from ammonia by electricity.

When the gas is generated and decomposed over the same mercury, there is always a greater expansion than the true one; and when the mercury is not boiled in the tube, and when common water is used, the nitrogen will be always over-rated, unless this error is counteracted by an opposite error, that of detonating with an excess of oxygen.*

Dr. Henry had the kindness to send me the apparatus, in which he conceived at that time, that he had witnessed the formation of water in the decomposition of ammonia by electricity, by his ingenious method of applying hygrometrical tests.

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* It will be seen by Dr. Henry's letter,† that in repeating his processes, since this paper was written, he has gained results almost precisely the same as those indicated in the text; and there is every reason to believe, that 100 of ammonia in volume uniformly become 180, when decomposed by electricity, and that the gas produced consists in 100 parts of 74 hydrogen and 26 nitrogen.

† Phil. Trans. 1809.

I tried one experiment only with it, and in this there seemed to me to be more moisture exhibited in the elastic matter after electrization than before, when it was cooled by the evaporation of ether: but on maturely considering this question, I do not think that the appearance of moisture even offers a decided proof of the existence of loosely combined oxygen in ammonia. To common hygrometrical tests, water must be less sensible in ammonia than in hydrogen or nitrogen, from its tendency to be precipitated in the form of alkaline solution, and likewise probably from its having a stronger adherence to the gas; and the elastic fluid generated, from the increase of volume, will be capable of containing more aqueous vapour.

It is not easy to determine, with perfect precision, the specific gravity of a gas, so light as hydrogen and even ammonia; but the loss of weight, which appears to take place in the electrical analysis of ammonia, cannot, I think, with propriety, be referred entirely to this circumstance; whether the solution that I have ventured to give* be the true one, I shall not, in the present state of the inquiry, attempt to discuss.

The question of ammonia being analogous to other salifiable bases in its constitution, is determined by the phenomena presented by the amalgam from that alkali; and if the conversion of nitrogen into oxygen and hydrogen should be established, it would appear that both hydrogen and nitrogen must be different combinations of ammonium with oxygen, or with water.

II. *Further Inquiries respecting Sulphur and Phosphorus.*

I have stated, in the last Bakerian Lecture, that hydrogen is produced from sulphur and phosphorus in such quantities, by Voltaic electricity, that it cannot well be considered as an accidental ingredient in these bodies. I have likewise stated, that when potassium is made to act upon them, the sulphurets and phosphurets evolve less hydrogen in the form of compound inflammable gas by the action of an acid, than the same quantity of potassium in an uncombined state, and from this circumstance, I have ventured to infer, that they may contain oxygen.

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* Bakerian Lecture, 1807, p. 97.

On the idea, that sulphur and phosphorus are deprived of some of their oxygen by potassium, it would follow, that when the compounds formed in this experiment are decomposed, these substances ought to be found in a new state; deoxygenated, as far as is compatible with their existence in contact with water.

With the view of examining the nature of the substances, separated by the action of muriatic acid upon the sulphurets and phosphurets of potassium, I combined a few grains of sulphur and phosphorus, with one-fourth of their weight of potassium, and exposed the compounds to the action of a strong solution of muriatic acid. As in the former cases, less inflammable gas was produced than would have been afforded by equal quantities of the uncombined potassium, and considerable quantities of solid matter separated from both compounds, which after being washed were collected in a filter.

The substance which separated from the sulphuret, was of a dark gray colour,* and was harsh to the touch; it had no taste, and at common temperatures no smell; but when heated, it emitted the peculiar odour of sulphur. Its specific gravity was rather less than that of sulphur. It softened at a low heat, so as to be moulded like wax between the fingers. It was a non-conductor of electricity. When heated upon a surface of glass, it soon fused, entered into ebullition, took fire, and burnt with the same light blue flame as sulphur. A small particle of it, made to combine with silver, presented the same phenomena as sulphur.

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* Possibly this colour may have been produced by the decomposition of a film of soap of naphtha adhering to the potassium.

The substance from the phosphuret was of an amber colour, and opaque. It could not be examined in the air, in the form in which it was collected (that of a loose powder) for as soon as it was wiped dry, it took fire, and burnt in the same manner as phosphorus; when melted under naphtha, it was found to differ from phosphorus, in being much deeper coloured, perfectly opaque, and very brittle. Its fusibility was nearly the same, and, like common phosphorus, it was perfectly non-conducting.

In experiments upon the union of potassium with sulphur and phosphorus the heat is so intense, that when larger quantities than a few grains are used, the glass tubes are uniformly fused or broken in pieces, and in consequence I have not been able to operate upon such a scale, as to make an accurate examination of the substances just described, and to determine the quantity of oxygen they absorb in being converted into acid. Metallic vessels of course cannot be employed; but I intend to try tubes of porcelain, in a further investigation of the subject.

It is evident that the sulphur and phosphorus, separated in these processes, are not in their common state; and the phenomena would certainly incline one to believe that they are less oxygenated. It may, I know, be said, that it is possible that they are merely combined with more hydrogen, and that the sulphur in this state is analogous to the hydrogenated sulphur of Berthollet, and to the alcohol of sulphur of Lampadius.

But when I decompounded dry sulphuret of potash by muriatic acid, of the same kind as had been used for decompounding the sulphuret of potassium, the substance produced seemed to be merely in that form, in which, according to the able researches of Dr. Thomson, it is combined with water; and notwithstanding the ingenious experiments of M. A. Berthollet and M. Robiquet,* the nature of the substance produced during the passage of sulphur over ignited charcoal is far from being fully ascertained. In a series of experiments, which my brother, Mr. John Davy, had the goodness to undertake, at my request, in the laboratory of the Royal Institution, on the action of sulphur on charcoal, the products were found to be very different, according as the charcoal employed differed in its nature. In an instance, in which imperfectly made charcoal was employed, the liquor that passed over left by combustion a residuum that had all the properties of carbonaceous matter, which agrees with the observations of MM. Desormes and Clement;† but when the charcoal had been well burnt, there was no such residuum produced. It was found, that the same charcoal might be employed in a number of processes till it was nearly entirely consumed, and that the sulphur, not rendered liquid, might be used for several operations. In all cases mixtures of ‡ sulphuretted hydrogen gas and hydrocarbonate were evolved.

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* Annales de Chimie, 1807, p. 144. 148.

† [That it is a compound of sulphur and carbon, is confirmed by later researches.]

‡ Five measures of the mixed gas, agitated with solution of potash, left a residuum of 3·5. These were detonated with 5·5 of oxygen; the whole diminution was to 6, of this residuum 2·5 appeared to be carbonic acid.

I particularly examined a specimen of the liquor which had been obtained in the last process from charcoal that had been often used. It was a non-conductor of electricity, and when the Voltaic spark was taken in it, did not evolve gas with more rapidity than sulphur; and this gas proved to be sulphuretted hydrogen.

Supposing the liquor to contain hydrogen in considerable quantities, I conceived that it must be decomposed by oxymuriatic acid; but it merely absorbed this substance, depositing crystals of common sulphur, and becoming a fluid similar to the sulphuretted muriatic acid; though when water was introduced, hydrated sulphur was instantly formed, and muriatic acid gas evolved.

From the quantity of carbonic acid formed by the combustion of the carburetted inflammable gas, produced in the operation of the action of well burnt charcoal upon sulphur, it may be conceived to contain oxygen. This circumstance, and the fact that no hydrate of sulphur or muriatic acid gas is formed by the operation of oxymuriatic acid upon the liquor, but common sulphur precipitated; are in favour of the opinion, that the sulphur in this liquor contains less oxygen than in its common state. This idea has likewise occurred to Dr. Marcet, who is engaged in some experiments on the subject, and from whose skill and accuracy, further elucidations of it may be expected.

III. *Further Inquiries respecting Carbonaceous Matter.*

On the idea which I have stated,* that the diamond may consist of the carbonaceous matter combined with a little oxygen, I exposed charcoal intensely ignited, by Voltaic electricity,* to nitrogen, conceiving it possible that if this body was an oxide, containing oxygen very intimately combined, it might part with it in small proportions to carbonaceous matter, and give an important result.

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* [Page 174.]

The charcoal which had been made with great care, was preserved for a quarter of an hour in a state of ignition, in which platina instantly fused. It did not appear to change in its visible properties; but a small quantity of black sublimate, which proved to be nothing more than finely divided carbonaceous matter, collected in an arborescent state upon the platina wire to which the charcoal was attached. The gas had increased in volume one-sixth; but this was owing to the evolution of carburretted inflammable gas from the charcoal; the nitrogen was unchanged in quantity, and as far as my examination could go, in quality. The points of the charcoal where the heat had been intense, were rather harder than before the experiment.

I have mentioned,† that charcoal, even when strongly ignited, is incapable of decomposing corrosive sublimate. When charcoal, in a state of ignition, is brought in contact with oxymuriatic acid gas, the combustion instantly ceases. I electrified two pieces of charcoal in a globe filled with oxymuriatic acid gas, which had been introduced after exhaustion of the globe. They were preserved, for nearly an hour, in intense ignition, by the same means that had been employed in the experiment on nitrogen. At first, white fumes arose, probably principally from the formation of common muriatic acid gas, by the action of the hydrogen of the charcoal upon the oxymuriatic acid, and the combination of the gas so produced, with aqueous vapour in the globe; but this effect soon ceased. At the end of the process, the oxymuriatic acid gas was found unaltered in its properties, and copper leaf burnt in it with a vivid light. The charcoal did not perceptibly differ from the charcoal that had been exposed to nitrogen. My view in making this experiment, was to ascertain whether some new combination of carbonaceous matter with oxygen might not be formed in the process, and I hoped likewise to be able to free charcoal entirely from combined hydrogen, and from alkaline and earthy matter, supposing they existed in it, not fully combined with oxygen. That hydrogen must have separated in the experiment, it is not possible to doubt, and on evaporating the deposit on the sides of the globe, which was in very minute quantity, and acted like concentrated muriatic acid, it left a perceptible saline residuum.*

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* The apparatus was the same as that referred to p. 160. The power employed was that of the battery of 500 belonging to the Royal Institution.

† [Page 202.]

IV. *Further Inquiries respecting Muriatic Acid.*

The experiments on muriatic acid, which I have already had the honour of laying before the Society, shew that the ideas which had been formerly entertained respecting the difference between the muriatic acid and the oxymuriatic acid are not correct. They prove that muriatic acid gas is a compound of a substance, which as yet has never been procured in an uncombined state, and from one-third to one-fourth of water, and that oxymuriatic acid is composed of the same substance, (free from water) united to oxygen. They likewise prove, that when bodies are oxidated in muriatic acid gas, it is by a decomposition of the water contained in that substance, and when they are oxidated in oxymuriatic acid, it is by combination with the oxygen in that body, and in both cases there is always a union of the peculiar unknown substance, the dry muriatic acid with the oxidated body.

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* Charcoal, over which sulphur has been passed, as in the experiments, page 219, as has been shown by M. A. Berthollet, contains sulphur, and this I find after being heated to whiteness; such charcoal is a conductor of electricity, and does not differ in its external properties from common charcoal.

Of all known substances belonging to the class of acids, the dry muriatic acid is that which seems to possess the strongest and most extensive powers of combination. It unites with all acid matters that have been experimented upon, except carbonic acid, and with all oxides (including water), and all inflammable substances that have been tried, except those which appear to be elementary, carbonaceous matter and the metals; and should its basis ever be separated in the pure form, it will probably be one of the most powerful agents in chemistry.

I have lately made several new attempts to procure uncombined dry muriatic acid; but they have been all unsuccessful.

I heated intensely, in an iron tube, silex in a very minute state of division, and muriate of soda that had been fused; but there was not the smallest quantity of gas evolved. In this case, the silex had been ignited to whiteness before it was used; but when silex in its common state was employed, or when aqueous vapour was passed over a mixture of dry silex and dry salt in a porcelain tube, muriatic acid gas was developed with great rapidity.

I have stated,* that a sublimate is formed by the combustion of the olive-coloured oxide of boracium in oxymuriatic acid. On the idea that this might be boracic acid, and that dry muriatic acid might be separated in the process, I examined the circumstances of the experiment; but I found the sublimate to be a compound of boracic and muriatic acid, similar to the compound of muriatic and phosphoric acid.

I heated freshly sublimed muriate of ammonia with potassium; when the quantities were equal, as much hydrogen gas was developed as is generated by the action of water on potassium; much ammonia was evolved, and muriate of potash formed; when the potassium was to the muriate as 4 to 1, less hydrogen appeared, and a triple compound of muriatic acid, ammonia, and potassium, or its protoxide was formed, which was of a dark gray colour, and gave ammonia and muriate of potash by the action of water. There was not the slightest indication of the decomposition of the acid in the experiment. The process, in which this decomposition may be most reasonably conceived to take place, is in the combustion of potassium in the phosphuretted muriatic acid, deprived by simple distillation with potassium of as much phosphorus as possible. I am preparing an apparatus for performing this experiment, in a manner which, I hope, will lead to distinct conclusions.

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* [Page 180.]

VI.

THE BAKERIAN LECTURE FOR 1809. ON SOME NEW ELECTRO-CHEMICAL RESEARCHES, ON VARIOUS OBJECTS, PARTICULARLY THE METALLIC BODIES, FROM THE ALKALIES, AND EARTHS, AND ON SOME COMBINATIONS OF HYDROGEN.*

I. *Introduction.*

I HAVE employed no inconsiderable portion of the time that has elapsed, since the last session of the Royal Society, in pursuing the train of experimental inquiries on the application of electricity to chemistry, the commencement and progress of which, this learned body has done me the honour to publish in their Transactions.

In this communication, I shall, as formerly, state the results. I hope they will be found to lead to some views and applications, not unconnected with the objects of the Bakerian Lecture: and though many of them are far from having attained that precision, and distinctness, which I could wish; yet still I flatter myself, that they will afford elucidations of some important and abstruse departments of chemistry, and tend to facilitate the progress of philosophical truth.

[* From Phil. Trans. for 1810. Read before Royal Society, Nov. 16th, 1809.]

II. *Some new Experiments on the Metals of the fixed Alkalies.*

In the paper, in which I first made known potassium and sodium to the Royal Society, I ventured to consider these bodies, according to the present state of our knowledge, as undecompounded, and potash and soda as metallic oxides, capable of being decomposed and recomposed, like other bodies of this class, and with similar phenomena.

Since that time, various repetitions of the most obvious of the experiments on this subject have been made in different parts of Europe. The generality of enlightened chemists, have expressed themselves satisfied both with the experiments, and the conclusions drawn from them: but as usually happens in a state of activity in science, and when the objects of inquiry are new, and removed from the common order of facts, some inquirers have given hypothetical explanations of the phenomena, different from those I adopted.

MM. Gay Lussac and Thenard, as I have mentioned on a former occasion, suppose potassium and sodium to be compounds of potash and soda, with hydrogen; a similar opinion seems to be entertained by M. Ritter. M. Curaudau* affects to consider them as combinations of charcoal, or of charcoal and hydrogen with the alkalies; and an inquirer† in our own country, regards them as composed of *oxygen* and *hydrogen.*

I shall examine such of these notions only, as have been connected with experiments, and I shall not occupy the time of the Society with any criticisms on matters of mere speculation.

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* Journal de Physique, June, 1808.

† Nicholson's Journal, August, 1809, p. 258.

In my two last communications, I have given an account of various experiments on the action of potassium upon ammonia, the process from which MM. Gay Lussac and Thenard derive their inferences. At the time that these papers were written, I had seen no other account of the experiments of the French chemists, than one given in a number of the Moniteur, and as this was merely a sketch, which I conceived might be imperfect, I did not enter into a minute examination of it. I have since seen a detail of their inquiry in the second volume of the Mem. d'Arcueil, a copy of which M. Berthollet has had the goodness to send me, and the publication of which is dated June 7, 1809; and from this detail, it seems that they still retain their opinion; but upon precisely the same grounds as those to which I have before referred. That no step of the discussion may be lost to the society, I shall venture to state fully their method of operation, and of reasoning.

They say that they heated potassium* in ammonia, and they found that a considerable quantity of ammonia was absorbed, and hydrogen produced; and that the potassium became converted into an olive-coloured fusible substance; by heating this substance strongly, they obtained three-fifths of the ammonia again, two-fifths as ammonia, one-fifth as hydrogen and nitrogen; by adding a little water to the residuum, they procured the remaining two-fifths, and found in the vessel in which the operation was carried on, nothing but potash. Again, it is stated, that by heating a new quantity of metal with the ammonia disengaged from the fusible substance, they again obtained hydrogen, and an absorption of the ammonia; and by carrying on the operation, they affirm, that they can procure from a given quantity of ammonia, more than its volume of hydrogen.

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* Mem. d'Arcueil, tom. ii. p. 309.

Whence, they ask, can the hydrogen proceed? shall it be admitted that it is from the ammonia? But this, say they, is impossible; for all the ammonia is reproduced. It must then come from the water which may be supposed to be in the ammonia, or from the metal itself. But the experiments of M. Berthollet, jun., prove that ammonia does not contain any sensible quantity of water. Therefore, say they, the hydrogen gas must be produced from the metal; and as when this gas is separated, the metal is transformed into potash, the metal appears to be nothing more than a combination of hydrogen and that alkali.

It is obvious, that even supposing the statement of these gentlemen correct, their conclusions may be easily controverted. They affirm that all the ammonia is reproduced; but they do not obtain it without the addition of *water*. And of the oxygen which this would give to the potassium, and of the hydrogen which it might furnish, to reproduce the ammonia, they take no notice.

I have shewn, by numerous experiments, many of which have been repeated before members of this society, that the results obtained by applying heat to the fusible substance, are very different from those stated by the ingenious French chemists, when the operations are conducted in a refined and accurate manner.

In proportion as more precautions are taken to prevent moisture from being communicated to it, so in proportion is less ammonia regenerated; and I have seldom obtained as much as $\frac{1}{10}$ of the quantity absorbed. And I have never procured hydrogen and nitrogen, in the proportions in which they exist in ammonia; but there has been always an excess of nitrogen.

The processes which I have detailed in the last Bakerian Lecture, and in the appendix to it, shew this; and they likewise shew that a considerable quantity of potassium is always revived.

I have lately performed the experiments, in a manner which I proposed, page 458 of the last volume of the Transactions,* and the results have been very satisfactory; as far as they relate to the question of the nature of potassium.

I employed a tube of platina bored from a single piece, which having a stop-cock and adaptor of brass, connected with the mercurial apparatus, could be used as a retort; the potassium was employed in quantities of from 3 to 4 grains, and the absorption of ammonia conducted as usual, in a retort of glass free from metallic oxides, and in a tray of platina.

In some of the processes, in which the heat was rapidly applied, some of the gray matter, which I have formerly described as a pyrophorus, passed over in distillation, and in those cases, there was a considerable deficiency of hydrogen, as well as nitrogen, in the results of the experiment; but when the heat was very slowly raised, the loss was much less considerable, and in several cases, I obtained more than four-fifths of the potassium which had been employed; and very nearly the whole of the nitrogen existing in the ammonia that had been acted upon.

I shall give an account of one process, conducted with scrupulous attention. The barometer was 30·2 in., thermometer at 54° Fahrenheit. Three grains and a half of potassium were heated in 12 cubical inches of ammonia, 7·5 were absorbed, and 3·2 of hydrogen evolved. The fusible substance was not exposed to the atmosphere, but was covered with dry mercury, and immediately introduced into the tube, which with its adaptors was exhausted, and filled with hydrogen. They contained together $\frac{8}{10}$ of a cubical inch. The heat was very slowly applied by means of a fire of charcoal, till the tube was ignited to whiteness. Nine cubical inches of gas were given off, and $\frac{1}{2}$ of a cubical inch remained in the retort and adaptors. Of the 9 cubical inches, $\frac{1}{5}$ of a cubical inch was ammonia, 10 measures of the permanent gas, mixed with 7·5 of oxygen, and acted upon by the electrical spark, left a residuum of 7·5. The quantity of potassium formed was such as to generate by its action upon water three cubical inches and $\frac{3}{10}$ of hydrogen gas.

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* [Page 213.]

Now if this experiment be calculated upon, it will be found, that 7·5 — ·2 = to 7·3 of ammonia, by its electrical decomposition, would afford about 13·1 of permanent gas, containing 3·4 of nitrogen, and 9·7 of hydrogen. But the 3·2 cubical inches of hydrogen, evolved in the first part of the process, added to the 5·8 evolved in the second part of the process = 9; and the nitrogen in the 8·8 cubical inches of gas, (or the 9 — ·2 of ammonia,) will be about 3; and if we estimate ·34 of hydrogen, and ·16 of nitrogen in the ·5 remaining in the retort; there will be very little difference in the results of the analysis of ammonia by electricity, and by the action of potassium; and calculating upon $\frac{8}{10}$ of hydrogen pre-existing in the tube and adaptors, the loss of hydrogen will be found proportionally rather greater than that of nitrogen.

In another experiment, in which 3 grains of potassium were employed in the same manner, 6·78 cubical inches of ammonia were found to be absorbed, and 2·48 of hydrogen only generated. The distillation was performed, the adaptors and tube being full of common air: 8 cubical inches of gas were produced; and there must have remained in the tubes and adaptors the same quantity of residual air as in the process last described.

The 8 cubical inches of gas contained scarcely $\frac{2}{5}$ of a cubical inch of ammonia; and the unabsorbable part detonated with oxygen, in the proportion of 11 to 6, gave a residuum of 7·5. The barometer was at 30·2 in., thermometer at 52° Fahrenheit. Dr. Pearson, Mr. Allen, and Mr. Pepys were present during the whole of these operations, and kindly assisted in the progress of them.

Now 6·78 — ·4 of ammonia = 6·38, and this quantity of gas decomposed by electricity, would afford 11·4 of permanent gas, consisting of 2·9 nitrogen, and 8·5 hydrogen; but there are produced in this experiment, of hydrogen 2·48 in the first operation, and 4·28 in the second, and considering the nitrogen in the permanent gas as 3·32, ·8 must be subtracted for the common air; which would give 2·52 for the nitrogen generated; and to these must be added, the quantity of hydrogen and nitrogen in the tubes and adaptors.

The quantity of potassium regenerated was sufficient to produce 2·9 cubical inches of hydrogen.

In all experiments of this kind, a considerable quantity of black matter separated, during the time the potassium in the tube was made to act upon water.

This substance was examined. It was in the state of a fine powder. It had the lustre of plumbago; it was a conductor of electricity. When it was heated, it took fire at a temperature below ignition; and after combustion, nothing remained but minutely divided platina.

I exposed some of it to heat in a retort, containing oxygen gas; there was a diminution of the gas; and a small quantity of moisture condensed in the upper part of the retort, which proved to be mere water.

I made two or three experiments, with a view to ascertain the quantity of this substance formed, and to determine more fully its nature. I found that in the process in which from 3 to 4 grains of potassium were made to act upon ammonia in a vessel of platina, and afterwards distilled in contact with platina, there were always from 4 to 6 grains of this powder formed; but I have advanced no further in determining its nature, than in ascertaining, that it is platina combined with a minute quantity of matter, which affords water by combustion in oxygen.

In the processes on the action of potassium and ammonia, in which iron tubes were used, as appears from the experiments detailed in the last Bakerian Lecture, and the appendix, there is always a loss of nitrogen, a conversion of a portion of potassium into potash, and a production of hydrogen. When copper tubes are employed, the hydrogen bears a smaller proportion to the nitrogen; and more potassium is revived.

In these experiments, in which platina has been used, there is little or no loss of potassium on nitrogen; but a loss, smaller or greater, of hydrogen.

It will be asked, on what do these circumstances depend? Does the affinity of certain metals for potassium prevent it from gaining oxygen from ammonia, and do platina and copper combine with a small quantity of hydrogen, or its basis? Or are there some sources of inaccuracy in those processes, in which nitrogen has appeared to be decomposed? The discussion of these difficult problems will be considered in that part of this lecture, in which the nature of ammonia will be illustrated by some new experiments. The object of the present part of the inquiry is the demonstration of a part of chemical doctrine, no less important and fundamental to a great mass of reasoning, namely, that by the operation of potassium upon ammonia, it is not *a metallic* body that is decompounded, but the volatile alkali, and that the *hydrogen* produced does not arise from the potassium, as is asserted by the French chemists, but from the *ammonia*, as I have always supposed; the potassium in the most refined experiments is recovered, but neither the ammonia nor its elements can be reproduced, except by introducing a new body, which contains oxygen and hydrogen.

I have made an experiment upon the action of sodium on ammonia, with the same precautions as in the experiments just detailed, a tray and the same tube of platina being employed.

3-$\frac{3}{10}$ grains of sodium I found absorbed 9·1 of ammonia, and produced about 4·5 of hydrogen, and the fusible substance, which was very similar to that from potassium, distilled, did not give off $\frac{1}{20}$ of the ammonia that had disappeared, and this small quantity I am inclined to attribute to the presence of moisture. The permanent gas produced equalled twelve cubical inches, and by detonation with oxygen, proved to consist of nearly two of hydrogen, to one of nitrogen. Sodium was regenerated, but an accident prevented me from ascertaining the quantity.

Whoever will consider with attention the more visible phenomena of the action of sodium on ammonia, cannot, I conceive, fail to be convinced that it is the volatile alkali, and not the metal which is decomposed in this process.

As sodium does not act so violently upon oxygen as potassium; and as soda does not absorb water from the atmosphere, with nearly so much rapidity as potash, sodium can be introduced into ammonia, much freer from moisture, than potassium. Hence, when it is heated in ammonia, there is no effervescence, or at least one scarcely perceptible. Its tint changes to bright azure, and from bright azure to olive green; it becomes quietly and silently converted into the fusible substance, which forms upon the surface, and then flows off into the tray. It emits no elastic fluid, and gains its new form, evidently, by combining with one part of the elementary matter of ammonia, whilst another part is suffered to escape in the form of hydrogen.

It will not be necessary for me to enter into a very minute experimental examination of the opinion of M. Curaudau, that the metals of the alkalies are composed of the *alkalies* merely united to *charcoal;* the investigation upon which he has founded his conclusions, is neither so refined, nor so difficult, as that which has been just examined. This gentleman has been misled by the existence of charcoal, as an accidental constituent in the metals he employed, in a manner much more obvious, than that in which MM. Gay Lussac and Thenard have been misled by the moisture which interfered with their results.

M. Curaudau states, that when sodium is oxidated, carbonic acid is formed. This I have never found to be the case, except when the sodium was covered by a film of naphtha. I burnt two grains of sodium in 8 cubical inches of oxygen: nearly two cubical inches of oxygen were absorbed, and soda in a state of extreme dryness, so that it could not be liquified by a heat below redness, formed. This soda did not give out an atom of carbonic acid, during its solution in muriatic acid. Three grains of sodium were made to act upon water; they decomposed it with the phenomena which I have described in the Bakerian Lecture for 1807. Nearly 6 cubical inches of hydrogen were produced. No charcoal separated; no carbonic acid was evolved, or found dissolved in the water. Whether the metals of potash or soda were formed by electricity, or by the action of ignited iron on the alkalies, the results were the same. When charcoal is used in experiments on potassium or sodium, they usually contain a portion of it in combination, and it appears from M. Curaudau's method of decomposing the alkalies, that his metals must have been carburets, not of potash and soda, but of potassium and sodium.

M. Ritter's argument in favour of potassium and sodium being compounds of hydrogen, is their extreme lightness. This argument I had in some measure anticipated, in my paper on the decomposition of the earths; no one is more easily answered. Sodium absorbs much more oxygen than potassium, and on the hypothesis of hydrogenation, must contain much more hydrogen; yet, though soda is said to be lighter than potash, in the proportion of 13 to 17 nearly,* yet sodium is heavier than potassium in the proportion of 9 to 7 at least.

On the theory which I have adopted, this circumstance is what ought to be expected. Potassium has a much stronger affinity for oxygen than sodium, and must condense it much more; and the resulting higher specific gravity of the combination is a necessary consequence.

M. Ritter has stated, that of all the metallic substances he tried for producing potassium by negative Voltaic electricity, tellurium was the only one by which he could not procure it. And he states the very curious fact, that when a circuit of electricity is completed in water, by means of two surfaces of tellurium, oxygen is given off at the positive surface; no hydrogen at the negative surface, but a brown powder, which he regards as a hydruret of tellurium, is formed and separates from it; and he conceives that the reason why tellurium prevents the metallization of potash is, that it has a stronger attraction for hydrogen than that alkali.

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* Hassenfratz, Annal. de Chim. tom. xxviii. p. 11.

These circumstances of the action of tellurium upon water are so different from those presented by the action of other metals, that they can hardly fail to arrest the attention of chemical inquirers. I have made some experiments on the subject, and on the action of tellurium on potassium, and I find that instead of proving that potassium is a compound of potash and hydrogen, they confirm the idea of its being as yet, like other metals, undecompounded.

When tellurium is made the positive surface in water, oxygen is given off; when it is made the negative surface, the voltaic power, being from a battery composed of a number of plates exceeding 300, a purple fluid is seen to separate from it, and diffuse itself through the water; the water gradually becomes opaque and turbid, and at last deposits a brown powder. The purple fluid is, I find, a solution of a compound of tellurium and hydrogen in water; which, in being diffused, is acted upon by the oxygen of the common air, dissolved in the water, and gradually loses a part of its hydrogen, and becomes a solid hydruret of tellurium. The compound of hydrogen and tellurium produced at the negative pole, when uncombined, is gaseous at common temperatures, and when muriatic acid, or sulphuric acid are present in the water, it is not dissolved, but is given off, and may be collected and examined.

I acted upon potash by means of a surface of tellurium, negatively electrified, by a part of the large Voltaic apparatus lately constructed on a new plan in the laboratory of the Royal Institution, an account of which, with figures, will be found annexed to this paper. 1000 double plates were used. The potash was in the common state, as to dryness. There was a most violent action, and a solution of the tellurium, with much heat, and a metallic mass, not unlike nickel in colour, was formed; which, when touched by water, did not inflame nor effervesce, but rendered the water of a beautiful purple colour, and when thrown into water, entirely dissolved, making a bright purple tincture. It immediately occurred to me, that the whole of the hydrogen, which in common cases would have been furnished from the decomposition of the water, had in this instance combined with the tellurium, and that the *telluretted* hydrogen (if the name may be used) had formed with the oxidated potassium, i. e. the potash, a peculiar compound, soluble in water; and this I found to be the case; for on pouring a little diluted muriatic acid into the mixture, it effervesced violently, and gave a smell very like that of sulphuretted hydrogen; metallic tellurium was formed where it came in contact with the air, and muriate of potash was found dissolved in the mixture.

It seemed evident from this fact, that in the action of tellurium negatively electrified upon potash, potassium was produced as in all other cases, and that it combined with the tellurium, and formed a peculiar alloy; and this opinion was farther confirmed, by the immediate action of potassium upon tellurium. When these metals were gently heated in a retort of green glass, filled with hydrogen gas, they combined with great energy, producing most vivid heat and light, and they composed an alloy of a dark copper hue, brittle, infusible at a heat below redness, and possessing a crystalline fracture. When the tellurium was in excess in this mixture, or even nearly equal to the potassium in quantity, no hydrogen was evolved by the action of the alloy upon water; but the compound of telluretted hydrogen and potash was formed, which remained dissolved in the fluid, and which was easily decomposed by an acid.

The very intense affinity of potassium and tellurium for each other, induced me to conceive that the *decomposition* of *potash*, might be easily effected, by acting on the oxide of tellurium and potash at the same time, by heated charcoal; and I soon proved that this was the case. About 100 grains of oxide of tellurium, and 20 of potash, were mixed with 12 grains of well-burnt charcoal in powder, and heated in a green glass retort; before the retort became red, there was a violent action, much carbonic acid was given off, a vivid light appeared in the retort, and there was found in it the alloy of tellurium and potassium.

In attempting to reduce some oxide of tellurium by charcoal, which Mr. Hatchett had the kindness to give me for the purposes of these experiments, and which must have been precipitated by potash, or from a solution in potash, I found that a sufficient quantity of alkali adhered to it, even after it had been well washed, to produce an alloy of potassium and tellurium; but in this alloy, the potassium was in very small quantity. It was of a steel gray colour, very brittle, and much more fusible than tellurium.

I shall not arrest the progress of discussion, by entering at present into a minute detail of the properties of the *aëriform* compound of tellurium and hydrogen; I shall mention merely some of its most remarkable qualities, and agencies, which, as will be shewn towards the close of this paper, tend to elucidate many points immediately connected with the subject in question.* The compound of tellurium and hydrogen is more analogous to sulphuretted hydrogen than to any other body. The smell of the two substances is almost precisely the same.† Its aqueous solution is of a claret colour; but it soon becomes brown, and deposits tellurium, by exposure to the air. When disengaged from an alkaline solution by muriatic acid, it reddens moistened litmus; but after being washed in a small quantity of water, it loses this property: but in this case likewise it is partially decomposed by the air in the water; so that it is not easy to say, whether the power is inherent in it, or depends upon the diffusion of a small quantity of muriatic acid through it. In other respects, it resembles a weak acid, combining with water, and with the alkalies. It precipitates most metallic solutions. It is instantly decomposed by oxymuriatic acid, depositing a film, at first metallic; but which is soon converted into muriate of tellurium.*

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* [This gas had not been previously known; the author, as in some other instances, in bringing it to light, not only made a discovery, but likewise obtained confirmation of his views by its operation in an experiment adduced to oppose them, the objector not having been aware of its existence.]

† In some experiments, made on the action of tellurium and potassium, in the laboratory of my friend John George Children, Esq., of Tunbridge, in which Mr. Children, Mr. Pepys, and Mr. Warburton cooperated, the analogy between the two substances struck us so forcibly, as for some time to induce us to conceive that *tellurium* might contain *sulphur*, not manifested in any other way but by the action of Voltaic electricity, or by potassium; and some researches made upon the habitudes of different metallic sulphurets, at the Voltaic negative surface, rather confirmed the suspicion; for most of the sulphurets that we tried, which were conductors of electricity, absorbed hydrogen in the Voltaic circuit. The great improbability, however, of the circumstance that sulphuric acid, or sulphur in any state of oxygenation could exist in a metallic solution, which was not manifested by the action of barytes, induced me to resist the influence; and further researches made in the laboratory of the Royal Institution, proved that the substance in question was a new and singular combination.

As arsenic has an affinity for hydrogen, it occurred to me as probable, that it would present some phenomena analagous to those offered by tellurium, in its action upon potassium, and in its operation upon water, when electrified.

Arsenic made the negative surface, in water, by means of a part of the new battery, containing 600 double plates, became dark-coloured, and threw down a brown powder; but it likewise gave off a considerable quantity of inflammable gas.

Arsenic negatively electrified in a solution of potash, likewise afforded elastic matter; but in this the whole solution took a deep tint of brown, and was pellucid; but it became turbid, and slowly deposited a brown powder, by the action of an acid. When arsenic was made the negative surface in contact with solid potash, an alloy of arsenic and potassium was formed of a dark gray colour, and perfectly metallic; it gave off arseniuretted hydrogen by the action of water, with inflammation, and deposited a brown powder.

When potassium and arsenic† were heated together in hydrogen gas, they combined with such violence as to produce the phenomena of inflammation, and an alloy was produced of the same kind as that formed by means of the Voltaic battery.

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* From the results of one experiment which I tried, it seems that tellurium, merely by being heated strongly in dry hydrogen, enters into combination with it. An accident prevented me from ascertaining whether the compound so formed is exactly the same as that described in the text.

† In reasoning upon the curious experiment of Cadet, of the pro-

As tellurium and arsenic both combine with hydrogen, it appeared to me probable, that by the action of alloys of potassium, with tellurium and arsenic, upon ammonia, some new phenomena would be obtained, and probably, still further proofs of the decomposition of the volatile alkali, in this process afforded; and this I found was actually the case.

When the easily fusible alloy of tellurium with potassium, in small quantity, was heated in ammonia, the surface lost its metallic splendour, and a dark brown matter was formed, which gáve ammonia by exposure to air; and the elastic fluid, which was generated in this operation, consisted of four-sixths nitrogen, instead of being pure hydrogen, as in the case of the action of potassium alone.

The alloy of arsenic and potassium, by its action upon ammonia, likewise produced a gas which was principally nitrogen, so that if it be said that the metal, and not the volatile alkali is decomposed in processes of this kind, it must be considered in some cases as a

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duction of a volatile pyrophorus by the distillation of acetite of potash, and white oxide of arsenic, (Fourcroy Connais. Chem. tom. viii. p. 197.) I conceived it probable, that this pyrophorus was a volatile alloy of potassium and arsenic. But from a repetition of the process, I find, that though potash is decompounded in this operation, yet that the volatile substance, is not an alloy of potassium, but contains charcoal and arsenic, probably with hydrogen. The gases not absorbable by water given off in this operation are peculiar. Their smell is intensely fetid. They are inflammable, and seem to contain charcoal, arsenic, and hydrogen: whether they are mixtures of various gases, or a single compound, I am not at present able to decide. [Very great caution is required in experimenting on this gas, on account of its highly poisonous quality.]

compound of nitrogen, and in others a compound of hydrogen, which are contradictory assumptions.

None of the chemists who have speculated upon the *imaginary hydrogenation* of potash, as far as my knowledge extends, have brought forward any arguments of analysis, or synthesis. Their reasonings have been founded, either upon distant analogies, or upon experiments in which agents, which they did not suspect, were concerned. No person, I believe, has attempted to shew that when potassium or sodium is burnt in oxygen gas, water is formed, or that water is generated when potassium decomposes any of the acids;* and no one has been able to form potassium by combining hydrogen with potash. I stated in the Bakerian Lecture for 1807, that when potassium and sodium were burnt in oxygen gas, the *pure* alkalies were formed in a state of extreme dryness; and that 100 parts of potassium, absorb about 18 parts of oxygen, and 100 parts of sodium about 34 parts. Though in the experiments from which these deductions were made, very small quantities only of the materials were employed, yet still from frequent repetitions of the process, I hoped that they would approach to accuracy; and I am happy to find that this is the case; for the results differ very little in some experiments which I have made, upon considerable portions of potassium and sodium, procured by chemical decomposition.

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* When, in October 1807, I obtained a dark coloured combustible substance from boracic acid, at the negative pole in the Voltaic circuit, I concluded that the acid was probably decomposed, according to the common law of electrical decomposition. In March 1808, I made further experiments on this substance, and ascertained that it produced acid matter by combustion; and I announced the decomposition in a public lecture delivered in the Royal Institution, March 12. Soon after I heated a small quantity of potassium, in contact with dry boracic acid, no water was given off in the operation, and I obtained the same substance as I had procured by electricity. MM. Gay Lussac and Thenard have likewise operated upon boracic acid, by potassium, and they conclude that they have decompounded it; but this does not follow from their theory, unless they prove that water is given off in the operation, or combined with the borate of potash; the legitimate conclusion to be drawn from the processes, on their hypothesis, was, that they had made a hydruret of boracic acid.

When potassium is burnt in trays of platina, in oxygen gas that has been dried by ignited potash, the absorption of oxygen is about $\frac{11}{20}$ of a cubical inch for every grain of the metal consumed; and when sodium is burnt in a similar manner, about a cubical inch is taken up for every grain.* The alkalies so formed, are only imperfectly fusible at a red heat; and do not, like the easily fusible alkalies, give indications of the presence of moisture.

M. D'Arcet has shewn by some very well conducted inquiries, that potash and soda,† in their common state, contain a considerable proportion of water; and M. Berthollet concludes, that 100 parts of potash, that have been kept for some time in fusion, contain 13·89 parts of water, which is lost when the alkali enters into combination with muriatic acid; and the same sagacious observer, from some very minute experiments, infers, that muriate of potash, which has been ignited, contains in the 100 parts 66·66 potash, and 33·34 muriatic acid, a determination which differs very little from that of Bucholz.

To determine the relation of dryness of the potash formed from potassium, to that which has been considered as freed from the whole, or the greater part of its water, in muriate of potash, I made several experiments. I first attempted to convert a certain quantity of potassium into potash, upon the surface of liquid muriatic acid; but in this case the heat was so intense, and hydrogen holding potassium in solution, was disengaged with so much rapidity, that there was a considerable loss of alkali; yet even under these circumstances, I obtained from 10 parts of potassium, 17·5 of dry muriate of potash. The most successful, and the only mode which I employed that can be entirely depended upon, was that of converting potassium into muriate of potash, in muriatic acid gas. I shall give the results of two experiments made in this manner, 5 grains of potassium inserted in a tray of platina, were made to act upon 19 cubical inches of muriatic acid gas, that had been exposed to muriate of lime; by the application of a gentle heat, the potassium took fire, and burnt with a beautiful red light;* and the whole mass appeared in igneous fusion; a little muriate of potash in the state of a white powder, sublimed and collected in the top of the vessel in which the experiment was made. Nearly 14 cubical inches of muriatic acid gas were absorbed, and about 5 of hydrogen were produced. The increase of weight of the tray was about 4·5 grains; and it did not lose any weight by being ignited.

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* The quantities of gas given out by the operation of water are in a similar ratio. See pages 144 of the last Bakerian Lecture, and page 235 of this paper.

† Annales de Chimie, Nov. 1808, p. 175.

The second experiment was conducted with still more attention to minuteness. 8 grains of potassium were employed; about 22 cubical inches of muriatic acid gas were consumed; the potassium burnt with the same brilliant phenomena as in the last experiment, and the increase of weight of the tray was 6½ grains. The muriate of potash was kept for some minutes in fusion in the tray, till a white fume began to rise from it, but it did not lose $\frac{1}{20}$ of a grain in weight. After the muriate of potash had been washed out of the tray, and it had been cleaned and dried, it was found to have lost about a third of a grain, which was platina in a metallic state, and that had alloyed with the potassium where it was in contact with the tray, during the combustion. There was no appearance of any water being separated in the process. A little muriate of potash sublimed; this was washed out of the retort, and obtained by evaporation; it did not equal $\frac{1}{8}$ of a grain.

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* As a retort exhausted of common air was used, the small quantity of residual common air may have been connected with this vividness of combustion.

Now if the data for calculation be taken from the last experiment, 8 grains of potassium will combine with 1·4 grains of oxygen, to form 9·4 grains of potash, and 6·6—1·4=5·2 the quantity of muriatic acid combined with the potash, which would give in the 100 parts in muriate of potash, 35·6 of acid, and 64·4 of potash; but 35·6 of muriatic acid, according to M. Berthollet's estimation, would demand 71·1 of alkali, in a state of dryness, in which it exists in muriate of potash, and 71·1—64·4=6·7,—so that the potash taken as a standard by M. Berthollet, contains at least 9 per cent. more water than that existing in the potash formed by the combustion of potassium in muriatic acid gas, which consequently may with much propriety be regarded as the dry alkali.*

After these illustrations, I trust the former opinions which I ventured to bring forward, concerning the metals of the fixed alkalies, will be considered as accurate, and that potassium and sodium, can with no more propriety be considered as *compounds* than any of the common *metallic substances*, and that potash and soda, as formed by the combustion of the metals, are pure metallic oxides, in which no water is known to exist.

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* Consequently M. Berthollet's fused potash must contain nearly 23 per cent. of water. From my own observations I am inclined to believe, that potash kept for some time in a red heat, contains 16 or 17 per cent. of water, taking the potash formed by the combustion of potassium as the dry standard.—[The same conclusion holds good considering the compound formed by the action of potassium on muriatic acid gas, as a chloride of potassium.]

These conclusions must be considered as entirely independent of hypothetical opinions, concerning the existence of hydrogen in combustible bodies, as a common principle of inflammability, and of intimately *combined water*, as an essential constituent of acids, alkalies, and oxides; this part of the inquiry I shall reserve for the conclusion of the lecture, and I shall first consider the nature of the metal ammonia, and the metals of the earths.

III. *Experiments on Nitrogen, Ammonia, and the Amalgam from Ammonia.*

One of the queries that I advanced, in attempting to reason upon the singular phenomena produced by the action of potassium upon ammonia was, that nitrogen might possibly consist of oxygen and hydrogen, or that it might be composed from water.

I shall have to detail, in this section, a great number of laborious experiments, and minute and tedious processes, made with the hopes of solving this problem. My results have been for the most part negative; but I shall venture to state them fully, because I hope they will tend to elucidate some points of discussion, and may prevent other chemists from pursuing the same paths of inquiry, and which at first view do not appear unpromising.

The formation of nitrogen has been often asserted to take place in many processes, in which none of its known combinations were concerned. It is not necessary to enter into the discussion of the ideas entertained by the German chemists on the origin of nitrogen, produced during the passage of water through red-hot tubes, or the speculations of Girtanner, founded on these and other erroneous data. The early discovery of Priestley on the passage of gases through red-hot tubes of earthenware, the accurate researches of Berthollet, and the experiments of Bouillon La-Grange, have afforded a complete solution of this problem.

One of the most striking cases, in which nitrogen has been supposed to appear without the presence of any other matter but water, which can be conceived to supply its elements, is in the decomposition and recomposition of water by electricity.* To ascertain if nitrogen could be generated in this manner, I had an apparatus made, by which a quantity of water could be acted upon by Voltaic electricity, so as to produce oxygen and hydrogen with great rapidity, and in which these gases could be detonated, without the exposure of the water to the atmosphere; so that this fluid was in contact with platina, mercury, and glass only; and the wires for completing the Voltaic and common electrical circuit, were hermetically inserted into the tube. 500 double plates of the Voltaic combination were used, in such activity, that about the eighth of a cubical inch of the mixed gases, upon an average, was produced from 20 to 30 times in every day. The water used in this experiment was about half a cubic inch; it had been carefully purged of air by the air-pump and by boiling, and had been introduced into the tube, and secured from the influence of the atmosphere whilst warm. After the first detonation of the oxygen and hydrogen, which together, equalled about the eighth of a cubical inch, there was a residuum of about $\frac{1}{40}$ of the volume of the gases; after every detonation this residuum was found to increase, and when about fifty detonations had been made, it equalled rather more than $\frac{1}{4}$ of the volume of the water, *i. e.* $\frac{1}{8}$ of a cubical inch. It was examined by the test of nitrous gas; it contained no oxygen; 6 measures mixed with 3 measures of oxygen, diminished to 5; so that it consisted of 2·6 of hydrogen, and 3·4 of a gas having the characters of nitrogen.

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* See Dr. Pearson's elaborate experiments on the decomposition of water by electrical explosions. Nicholson's Journal, 4to. vol. i. page 301.

This experiment seemed to favour the idea of the production of nitrogen from pure water in these electrical processes; but though the platina wires were hermetically sealed into the tube, it occurred to me as possible, that at the moment of the explosion by the electrical discharge, the sudden expansions and contractions might occasion some momentary communications with the external air through the aperture; and I resolved to make the experiment in a method by which the atmosphere was entirely excluded. This was easily done by plunging the whole of the apparatus, excepting the upper parts of the communicating wires, under oil, and carrying on the process as before. In this experiment the residuum did not seem to increase quite so fast as in the former one. It was carried on for nearly two months. After 340 explosions, the permanent gas equalled $\frac{24}{100}$ of a cubical inch. It was carefully examined: six measures of it detonated with three measures of oxygen, diminished to rather less than one measure. A result which seems to show, that nitrogen is not formed during the electrical decomposition and recomposition of water, and that the residual gas is hydrogen. That the hydrogen is in excess, may be easily referred to a slight oxidation of the platina.

The refined experiments of Mr. Cavendish on the deflagration of mixtures of oxygen, hydrogen, and nitrogen, lead directly to the conclusion, that the nitrous acid sometimes generated in experiments on the production of water, owes its origin to nitrogen, mixed with the oxygen and hydrogen, and is never produced from these two gases alone. In the Bakerian Lecture for 1806, I have stated several facts, which seem to show that the nitrous acid, which appears in many processes of the Voltaic electrization of water, cannot be formed, unless *nitrogen* be present.

Though in these experiments I endeavoured to guard with great care against all causes of mistake, and though I do not well see how I could fall into an error; yet I find that the assertion, that both acids and alkalies may be produced from pure water, has been again repeated.* The energy with which the large Voltaic apparatus, recently constructed in the Royal Institution, acts upon water, enabled me to put this question to a more decided test than was before in my power. I had formerly found in an experiment, in which pure water was electrified in two gold cones in hydrogen gas, that no nitrous acid nor alkali was formed. It might be said, that in this case the presence of hydrogen dissolved in the water, would prevent nitrous acid from appearing; I therefore made two series of experiments, one in a jar filled with oxygen gas, and the other in an apparatus, in which glass, water, mercury, and wires of platina only were present.

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* Nicholson's Journal, August, 1809, p. 258.

In the first series 1000 double plates were used, the two cones were of platina, and contained about $\frac{1}{12}$ of a cubical inch each, and filaments of asbestus were employed to connect them together. In these trials, when the batteries were in full action, the heat was so great, and the gases were disengaged with so much rapidity, that more than half the water was lost in the course of a few minutes. By using a weaker charge, the process was carried on for some hours, and in some cases, for from two to three days. In no instance, in which slowly distilled water was employed, and in which the receiver was filled with pure oxygen, from oxymuriate of potash, was an acid or alkali exhibited in the cones; even when nitrogen was present, the indications of the production of acid and alkaline matter were very feeble; though if the asbestus was touched with unwashed hands, or the smallest particle of neutro-saline matter introduced, there was an immediate separation of acid and alkali at the points of contact of the asbestus with the platina, which could be made evident by the usual tests.

In the second series of experiments, the oxygen and hydrogen produced from the water were collected under mercury, and the two portions of water communicated directly with each other. In several trials made in this way, with a combination of 500 plates, and continued for some days, it was always found that fixed alkali separated in the glass negatively electrified: and a minute quantity of acid, which could barely be made evident by litmus, in the glass positively electrified. This acid rendered cloudy nitrate of silver. Whether its presence was owing to impurities which might rise in distillation with the mercury, or to muriatic acid existing in the glass, I cannot say; but as common salt perfectly dry is not decomposed by silex, it seems very likely that muriatic acid in its arid state may exist in combination in glass.*

I tried several experiments on the ignition and fusion of platina by Voltaic electricity, in mixtures of the vapour of water and oxygen gas. I thought it possible, if water could be combined with *more oxygen*, that this heat, the most intense we are acquainted with, might produce the effect. When the oxygen was mixed with nitrogen, nitrous acid was formed: but when it consisted of the last portions from oxymuriate of potash, there was not the slightest indication of such a result.

Water in vapour was passed through oxide of manganese, made red hot in a glazed porcelain tube, the base of which was nearly an inch in diameter; in this case a solution of nitrous acid, sufficiently strong to be disagreeably sour to the taste, and which readily dissolved copper, was formed.

This experiment was repeated several times, and when the diameter of the tube was large, with precisely the same results. When red oxide of lead was used instead of oxide of manganese, no acid however was generated; but upon this substance a single trial only was made, and that in a small tube, so that no conclusion can with propriety be drawn from this failure.

I stated in the last Bakerian Lecture, that in attempting to produce ammonia from a mixture of charcoal and pearlash, that had been ignited by the action of water in the manner stated by Dr. Woodhouse, I failed in the trial in which the mixture was cooled in contact with hydrogen. I have since made a number of similar experiments. In general, when the mixture had not been exposed to air, there was little or no indication of the production of the volatile alkali; but the result was not so constant as to be entirely satisfactory; and the same circumstances could not be uniformly obtained in this simple form of the experiments. I had an apparatus made, in which the phenomena of the process could be more rigorously examined. Pure potash and charcoal, in the proportion of one to four in weight, were ignited in the middle of a tube of iron, furnished with a system of stop-cocks, and connected with a pneumatic apparatus, in such a manner, that the mixture could be cooled in contact with the gas, produced during the operation; and that water exhausted of air, could be made to act upon the cooled mixture, and afterwards, distilled from it; figures of this apparatus, and an account of the manner in which it was used, are annexed to this paper. In this place I shall state merely the general results of the operations, which were carried on for nearly two months, a variety of precautions being used to prevent the interference of nitrogen from the atmosphere.

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* [Or, if not in combination, at least in mechanical mixture. In accordance with this it may be mentioned, that salt is found to exist in the majority of secondary rocks, and in volcanic rocks which have been elevated in the sea. In some of the Lipari islands I have witnessed thin layers of sea-salt between the rocky strata; applying the term to a structure so coarsely lamellar as to resemble stratification, particularly in Felicuda.]

In all cases in which the water was brought in contact with the mixture of charcoal and potash, when it was perfectly cool, and afterwards distilled from it by a low heat, it was found to hold in solution, small quantities of ammonia; when the operation was repeated upon the same mixture, ignited a second time, the proportion diminished; in a third operation it was sensible, but in the fourth barely perceptible. The same mixture, however, by the addition of a new quantity of potash, again gained the power of producing ammonia in two or three successive operations, and when any mixture had ceased to give ammonia, the power was not restored by cooling it in contact with air.

Ammonia was produced in a case in which more than 200 cubical inches of gas had passed over from the action of water upon a mixture, and when the last portions only were preserved in contact with it during the cooling. In a comparative trial, it was found, however, that considerably more ammonia was produced, when a mixture was cooled in contact with the gas developed in the operation.

I shall not attempt to draw any conclusions from these processes. It would appear from some experiments of M. Berthollet, that nitrogen adheres very strongly to charcoal.* The circumstance, that the ammonia ceases to be produced after a certain number of operations, and that the quantity is much greater when free nitrogen is present, are perhaps against the idea that nitrogen is composed in the process. But till the weights of the substances concerned and produced in these operations are compared, no correct decision on the question can be made.

The experiments of Dr. Priestley upon the production of nitrogen, during the freezing of water, induced that philosopher to conceive, either that water was capable of being converted into nitrogen, or that it contained much more nitrogen than is usually suspected.

I have made some repetitions of his processes. A quantity of water, (about a cubical inch and a quarter), that had been produced from snow, boiled and inverted over mercury whilst hot, was converted into ice, and thawed in 16 successive operations; gas was produced, but after the first three or four times of freezing, there was no notable increase of the volume. At the end of the experiment, about $\frac{1}{30}$ of a cubical inch was obtained, which proved to be common air.

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* Mem. d'Arcueil, tom. ii. p. 485.

About four cubical inches of water from melted snow were converted into ice and thawed four successive times in a conical vessel of wrought iron. At the end of the fourth process, the volume of gas equalled about $\frac{1}{20}$ of the volume of the water. It proved to contain about $\frac{1}{10}$ oxygen, $\frac{3}{10}$ hydrogen, and $\frac{6}{10}$ nitrogen.

Mr. Kirwan observed the fact that when nitrous gas, and sulphuretted hydrogen, are kept in contact for some time, there is a great diminution of volume, and that the nitrous gas becomes converted into nitrous oxide, and that sulphur is deposited which has an ammoniacal smell. I repeated this experiment several times in 1800, with similar results, and I found, that the diminution of the volume of the gases when they were mixed in equal proportions, was rather less than $\frac{1}{4}$, which seemed to be nitrous oxide.

In reasoning upon this phenomena, I saw grounds for a minute investigation of it. Sulphuretted hydrogen, as appears from experiments which I have stated on a former occasion, and from some that I shall detail towards the conclusion of this lecture, contains a volume of hydrogen, equal to its own. But one of hydrogen demands half its volume of oxygen to convert it into water, and nitrous gas consists of about half a part in volume of oxygen; so that supposing the whole of the hydrogen employed in absorbing oxygen from nitrous gas, nitrogen alone ought to be formed, and no nitrous oxide. Or if the whole of the gas is nitrous oxide, this should contain all the nitrogen of the nitrous gas, leaving none to be supplied to the ammonia. I mixed together five cubical inches of nitrous gas and five of sulphuretted hydrogen over mercury, the barometer being at 29·5 in., thermometer at 51° Fahrenheit; twelve hours had elapsed before any change was perceived; there was then a whitish precipitate formed, and a deep yellow liquid began to appear in drops on the inside of the jar, and the volume of the gases quickly diminished; after two days the diminution ceased, and the volume became stationary; the barometer was at 30·45 in., and thermometer 52° Fahrenheit; when it equalled 2·3. The gas proved to be about ¾ nitrous oxide, and the remaining fourth was inflammable. An experiment was made expressly to determine the nature of the deep yellow liquid in the jar. It proved to be of the same kind as Boyle's fuming liquor, the hydro-sulphuret of ammonia, but with sulphur in great excess.

In this experiment there was evidently no formation of nitrogen, and these complicated changes ended in the production of two new compounds; nitrogen, hydrogen; oxygen and sulphur combining to form one; and a part of the nitrogen and oxygen, becoming more condensed, to form another.

Having stated the results of the investigation on the production of nitrous acid and ammonia, in various processes of chemistry, I shall notice some attempts that I made to decompound nitrogen by agents, which I conceived might act at the same time on oxygen, and on the basis of nitrogen. Potassium, as I have before stated, sublimes in nitrogen, without altering it, or being itself changed: but I thought it possible, that the case might be different, if this powerful agent were made to act upon nitrogen, assisted by the intense heat and decomposing energy of Voltaic electricity.

I had an apparatus made, by which the Voltaic circuit could be completed in nitrogen gas, confined by mercury, by means of potassium and platina. The potassium, in the quantity of about two or three grains, was placed in a cup of platina, and by contact with a wire of platina, it could be fused and sublimed in the gas. The quantity of nitrogen was usually about a cubical inch. The battery employed was always in full action for these experiments, and consisted of one thousand double plates. The phenomena were very brilliant; as soon as the contact with the potassium was made, there was always a bright light, so intense as to be painful to the eye; the platina became white hot; the potassium rose in vapour; and by increasing the distance of the cup from the wire, the electricity passed through the vapour of the potassium, producing a most brilliant flame, of from half an inch to an inch and a quarter in length; and the vapour seemed to combine with the platina, which was thrown off in small globules in a state of fusion, producing an appearance similar to that produced by the combustion of iron in oxygen gas.

In all trials of this kind, hydrogen was produced; and in some of them there was a loss of nitrogen. This at first seemed to lead to the inference, that nitrogen is decompounded in the process; but I found that, in proportion as the potassium was introduced more free from a *crust of potash*, which would furnish water, and consequently hydrogen in the experiment, so in proportion was there less of this gas evolved; and in a case in which the greatest precautions were taken, the quantity did not equal $\frac{1}{8}$ of the volume of gas, and there was no sensible quantity of nitrogen lost.

The largest proportion of nitrogen which disappeared in any experiment, was $\frac{1}{11}$th of the quantity used; but in this case the crust of potash was considerable, and a volume of hydrogen, nearly equal ¼th of the nitrogen was produced. It cannot be said, that the nitrogen is *not* decomposed in this operation; but it seems much more likely that the slight loss is owing to its combination with nascent hydrogen, and its being separated with the potassium in the form of the gray pyrophoric sublimate, which I have found is always produced, when potassium is electrized and converted into vapour in ammonia.

The phosphuret of lime in its common state is a conductor of electricity; and when it was made the medium of communication between the wires of the great battery, it burnt with a most intense light. It was ignited to whiteness in nitrogen gas; a little phosphuretted hydrogen was given off from it, but the nitrogen was not altered; the apparatus was similar to that used for the potassium.

As almost all compounds known to contain hydrogen are readily decomposed by oxymuriatic acid gas, a mixture of nitrogen and oxymuriatic acid gas was passed through a porcelain tube heated to whiteness; the products were received in a pneumatic apparatus over water; there was a small loss of nitrogen, but the greatest part came over densely clouded, and as nitro-muriatic acid was found dissolved in the water, no conclusions concerning the decomposition of nitrogen can be drawn from the process.

The general tenor of these inquiries cannot be considered as strengthening in any considerable degree the suspicion which I formed of the decomposition of nitrogen, by the distillation of the olive-coloured substance from potassium and ammonia, in tubes of iron.

In reasoning closely upon the phenomena of the operation, it appears to me indeed possible to account for the loss of nitrogen, without assuming that it has been converted into new matter. Though the iron tubes which I used were carefully cleaned; yet still it was not unlikely that a small quantity of oxide might adhere to the welded parts; the oxygen of which, in the beginning of the process of distillation, might form water with hydrogen, given off from the fusible substance; which being condensed in the upper part of the tube, would be again brought into action towards the close of the operation, occasioning the formation, and possibly the absorption of some ammonia, and consequently a loss of nitrogen, and the production of an increased proportion of hydrogen. I have made an experiment with the hopes of deciding this question in an iron tube, used immediately after the whole internal surface had been cleaned by the borer; six grains of potassium were used in a tray of iron, nearly thirteen cubical inches of ammonia were absorbed, and about six of hydrogen produced. Thirteen cubical inches of gas were evolved in the first operation, which consisted of nearly one cubical inch of ammonia, 4 of nitrogen, and 8 of hydrogen. The portion of gas given off in the second operation, equalled 3·6 cubical inches; which consisted of 2·5 hydrogen, and 1·1 nitrogen. The potassium produced in the operation, was sufficient to generate 3·1 cubical inches of hydrogen.

As the iron in these experiments had been heated to intense whiteness, and must have been very soft, it was not impossible, considering the recent experiments of M. Hassenfratz,* that the loss of so large a proportion of potassium might depend upon an intimate union of that body with iron, and its penetration into the substance of the tube. This idea is countenanced by another experiment of the same kind, in which the heat was raised to whiteness, and the barrel cut into pieces when cool: on examining the lower part of it, I found in it a very thin film of potash; but which I conceive could scarcely equal a grain in weight. The pieces of the barrel were introduced under a jar inverted in water; at the end of two days, nearly 2·3 cubical inches of hydrogen were found to be generated.

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* Journal des Mines, Avril, 1808, p. 275.

In the experiments detailed in page 154, a loss of nitrogen, and a production of hydrogen was perceived in a case in which the residuum, from a portion of fusible substance which had been exposed to a low red heat, was distilled in a tube of platina; but in this case the residuum had been covered by *naphtha*, and it is possible that ammonia might have been regenerated by hydrogen from the naphtha, and absorbed by that fluid; and a part of the hydrogen might likewise proceed from the decomposition of the naphtha; and in several experiments in which I have burnt the entire fusible substance, I have found no loss of nitrogen.

Even the considerable excess of hydrogen, and deficiency of nitrogen, in the processes in which the fusible substance is distilled with a new quantity of potassium, page 209, it is possible to refer to the larger quantity of moisture, which must be absorbed by the fusible substance from the air during the time occupied in attaching the potassium to the tray, and likewise from the moisture adhering to the crust of potash, which always forms upon the potassium during its exposure to air.

These objections are the strongest that occur to me against the mode of explaining the phenomena, by supposing nitrogen decomposed in the operation; but they cannot be considered as decisive on this complicated and obscure question, and the opposite view may be easily defended.

Though I have already laid before the society a number of experiments upon the decomposition of ammonia; yet I shall not hesitate to detail some further operations which have been conducted according to new views of the subject.

I concluded from the loss of weight taking place in the electrical analysis of ammonia, that water or oxygen was probably separated in this operation; but I was aware that objections might be made to this mode of accounting for the phenomena.

The experiment of producing an amalgam from ammonia, which regenerated volatile alkali, apparently by oxidation, confirmed the notion of the existence of oxygen in this substance, at the same time it led to the suspicion, that of the two gases separated by electricity, one, or perhaps both, might contain metallic matter united to oxygen; and the results of the distillation of the fusible substance from potassium and ammonia, notwithstanding the objection I have made, can, perhaps, be explained on such a supposition.

I have made a number of experiments upon the decomposition of considerable quantities of ammonia, both by Voltaic and common electricity; and I have used an apparatus (of which a figure is attached to this paper) in which nothing was present but the gas, the metals for conveying the electricity, and glass. The ammonia was introduced by a stop-cock which was cleared of common air, into a globe that was exhausted, after being filled two or three times with ammonia: the gas that was used was absolutely pure; the decomposition was performed without any possibility of charge in the volume of the elastic matter, and the apparatus was such, that the gas could be exposed to a *freezing mixture*, and the whole weighed before and after the experiment.

The object in keeping the volume the same during the decomposition, was to produce the condensation of any aqueous vapour which, if formed in small quantity in the operation (on the theory of the mechanical diffusion of vapour in gases) might in the common case of decomposition, under the usual pressure, be in quantity nearly twice as much in the hydrogen and nitrogen, as in the ammonia.

In all instances it was found that there was no loss of weight of the apparatus, nor was there any deposition of moisture, during or after the electrization; but the wires were uniformly tarnished; and in an experiment in which surfaces of brass were used, a small quantity of olive-coloured matter formed on the metal; but though in this case nearly 8 cubical inches of ammonia were decomposed, the weight of the oxidated matter was so minute as to be scarcely sensible. By the use of a freezing mixture of muriate of lime and ice, which diminished the temperature to—15°, there was a very feeble indication given of the addition of hygrometrical moisture.

In these experiments the increase of the gas was uniformly (within a range of five parts) from 100 to 185, the hydrogen was to the nitrogen in the average proportions of from 73·74 to 27·26; the proper corrections being made, and the precautions before referred to being taken.*

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* Philosophical Transactions 1809, p. 459. M. Berthollet, junior, in the 2d volume of the Memoirs of Arcueil, has given a paper on the decomposition of ammonia, and he enters into an examination of my idea of the oxygen, separated in the electrical decomposition of ammonia, which he supposes I rate at 20 per cent. and at the same time he con-

Assuming the common estimations of the specific gravity of ammonia, of hydrogen, and nitrogen, the conclusions which I have advanced in the Bakerian Lecture for 1807, would be supported by these new experiments, but as the moisture and oxygen visibly separated cannot be conceived to be as much as $\frac{1}{11}$ or $\frac{1}{12}$ of the weight of the ammonia, I resolved to investigate more precisely, than I had reason to think had been hitherto done, the specific gravities of the gases concerned in their dry state; and the very delicate balance belonging to the Royal Institution placed the means of doing this in my power. Nitrogen, hydrogen, and ammonia, were dried by a long continued exposure to potash, and were very carefully weighed. Their relative specific gravities proved to be at 30·5 barometer, 51° Fahrenheit's thermometer.

For nitrogen, the 100 cubical inches .	29·8 grains.
For hydrogen, ditto	2·27
For ammonia	18·4

futes some experiments which he is pleased to attribute to me of the combustion of charcoal and iron in ammonia. His arguments and facts upon these points, appear to me perfectly conclusive; but as I never formed such an opinion, as that 20 of oxygen were separated in the experiments, and never imagined such results as the combustion of iron and charcoal in ammonia, and never published any thing which could receive such an interpretation, I shall not enter into any criticism on this part of his paper. The experiments of this ingenious chemist on the direct decomposition of ammonia, seem to have been conducted with much care, except as to the circumstance of his not boiling the quicksilver; which I conceive has occasioned him to overrate the increase of volume. At all events a loss of weight is more to be expected than an increase of weight, in all very refined experiments of this kind. It is possible that the volume may be exactly doubled, and that the nitrogen may be to the hydrogen as one to three; but, neither the numerous experiments of Dr. Henry, nor those that I have tried, establish this; it is one of the hypothetical inferences that may be made, but it cannot be regarded as an absolute fact.

Now, if these data be calculated upon, it will be found, that in the decomposition of 100 of ammonia, taking even the largest proportions of gases evolved, there is a loss of $\frac{1}{18}$,* and if the smallest proportion be taken the loss will be nearly $\frac{1}{13}$.

These results and calculations agree with those that I have before given, and with those of Dr. Henry.

The lately discovered facts in chemistry, concerning the important modifications which bodies may undergo by very slight additions or subtractions of new matter, ought to render us cautious in deciding upon the nature of the process of the electrical decomposition of ammonia.

It is *possible*, that the minute quantity of oxygen which appears to be separated is not accidental, but a result of the decomposition; and if hydrogen and nitrogen be both oxides of the same basis, the possibility of the production of different proportions of water, in different operations, might account for the variations observed in some cases in their relative proportions; but on the whole, the idea that ammonia is decomposed into hydrogen and nitrogen alone, by electricity, and that the loss of weight is no more than is to be expected in processes of so delicate a kind, is in my opinion the most defensible view of the subject.

But if *ammonia* be capable of decomposition into nitrogen and hydrogen, what, it will be asked, is the nature of the matter existing in the amalgam of ammonia? what is the metallic basis of the volatile alkali? These are questions intimately connected with the whole of the arrangements of chemistry; and they are questions, which, as our instruments of experiment now exist, it will not, I fear, be easy to solve.

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* 100 of ammonia at the rate of 185, will give 136·9 of hydrogen weighing 3·1 grains, and 48·1 of nitrogen weighing 14·33 grains; but 18·4—17·4=1, and at the rate of 180, 133 of hydrogen weighing 3·01 and 47 of nitrogen, weighing 14 and 18·4—17=1.4.

I have stated in my former communication on the amalgam from ammonia, that under all the common circumstances of its production, it seems to preserve a quantity of water adhering to it, which may be conceived to be sufficient to oxidate the metal, and to reproduce the ammonia.

I have tried various devices with the hopes of being able to form it from ammonia in a dry state, but without success. Neither the amalgams of potassium, sodium, or barium, produce it in ammoniacal gas; and when they are heated with muriate of ammonia, unless the salt is moist, there is no metallization of the alkali.

I have acted upon ammonia by different metallic amalgams negatively electrified, such as the amalgams of gold and silver, the amalgam of zinc, and the liquid amalgam of bismuth and lead; but in all these cases, the effect was less distinct, than when pure mercury was used.

By exposing the mercury to a cold of—20° Fahrenheit in a close tube, I have succeeded in obtaining an amalgam in a much more solid state; yet this decomposed nearly as rapidly as the common amalgam, but it gave off much more gaseous matter; and in one instance I obtained a quantity which was nearly equal to six times its volume.

The amalgam which I have reason to believe can be made most free from *adhering moisture*, is that of potassium, mercury, and ammonium in a solid state. This, as I have mentioned in my former communication, decomposes very slowly, even in contact with water, and when it has been carefully wiped with bibulous paper, bears a considerable heat without alteration. I have lately made several new attempts to distil the ammonium from it, but without success. When it is strongly heated in a green glass tube filled with hydrogen gas, there is always a partial regeneration of ammonia; but with this ammonia there is from $\frac{5}{10}$ to $\frac{6}{10}$ of hydrogen produced.

As it does not seem possible to obtain an amalgam in an uniform state, as to adhering moisture, it is not easy to say what would be the exact ratio between the hydrogen and ammonia produced, if no more water was present, than would be decomposed in oxidating the basis. But in the most refined experiments which I have been able to make, this ratio is that of one to two; and in no instance in which proper precautions are taken, is it less; but under common circumstances often more. If this result is taken as accurate, then it will follow, that ammonia (supposing it to be an oxide) must contain about 48 per cent. of oxygen, which, as will be hereafter seen, will agree with the relations of the attractions of this alkali for acids, to those of other salifiable bases.*

If hydrogen be supposed to be a simple body, and nitrogen an oxide, then on the hypothesis above stated, nitrogen would consist of nearly 48 of oxygen, and 34 of basis; but if the opinion be adopted, that hydrogen and nitrogen are both oxides of the same metal, then the quantity of oxygen in nitrogen must be supposed less.

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* Even in common air, the amalgam evolves hydrogen and ammonia nearly in these proportions; and in one experiment which I lately tried, there appeared to be no absorption of oxygen from the atmosphere. This circumstance appears to me in favour of the antiphlogistic view of the metallization of the volatile alkali; for if the hydrogen be supposed to be given off from the mercury, and not to arise from the decomposition of water adhering to the amalgam, it might be conceived that, being in the nascent state, it would rapidly absorb oxygen. In my first experiment upon the amalgam, finding that common air, to which it had been exposed, gave less diminution with nitrous gas than before, I concluded naturally, that oxygen had been absorbed; but this difference might have arisen, partly, at least, from the mixture of hydrogen. Whether, in any case, the amalgam absorbs oxygen gas, is a question for further investigation.

These views are the most obvious that can be framed on the antiphlogistic hypothesis of the nature of metallic substances: but if the facts concerning ammonia were to be reasoned upon, independently of the other general phenomena of chemical science, they perhaps might be more easily explained on the notion of nitrogen being a basis, which became alkaline by combining with one portion of hydrogen, and metallic, by combining with a greater proportion.

The solution of the question concerning the quantity of matter added to the mercury in the formation of the amalgam, depends upon this discussion; for if the phlogistic view of the subject be adopted, the amalgam must be supposed to contain nearly twice as much matter as it is conceived to contain on the hypothesis of deoxygenation. In the last Bakerian Lecture, I have noted the proportion at $\frac{1}{12000}$, but this is the least quantity that can be assumed; the mercury being supposed to give off only one and a half its volume of ammonia. If the proportion stated in page 265 be taken as the basis of calculation, which is the maximum that I have obtained, the amalgam would contain about $\frac{1}{16000}$ of new matter on the antiphlogistic view, and about $\frac{1}{900}$ on the phlogistic view.

I shall have occasion to recur to, and to discuss more fully these ideas; and I shall conclude this section by stating, that though the researches on the decomposition and composition of nitrogen, which have occupied so large a space in the foregoing pages, have been negative, as to the primary object; yet they may not possibly be devoid of useful applications. It does not seem improbable that the passage of steam over hot manganese, may be applied to the manufacture of nitrous acid. And there is reason to believe that the ignition of charcoal and potash, or their exposure to water, may be advantageously applied to the production of volatile alkali, in countries where fuel is cheap.

IV. *On the Metals of Earths.*

I have tried a number of experiments with the hope of gaining the same distinct evidences of the decomposition of the common earths, as those afforded by the electro-chemical processes on the alkalies, and on the alkaline earths. I find that when iron wire ignited to whiteness by the power of 1000 double plates, is negatively electrified, and fused in contact with either silex, alumine, or glucine, slightly moistened and placed in hydrogen gas, the iron becomes brittle and whiter, and affords by solution in acids, an earth of the same kind as that which has been employed in the experiment.

I have passed potassium in vapour through each of these earths, heated to whiteness in a platina tube: the results were remarkable, and perhaps not unworthy of being fully detailed.

When silex was employed, being in the proportion of about ten grains to four of potassium, no gas was evolved, except the common air of the tube mingled with a little inflammable gas, not more than might be referred to the moisture in the crust of alkali formed upon the potassium. The potassium* was entirely destroyed; and glass with excess of alkali was formed in the lower part of the tube. When this glass was powdered, it exhibited dark specks, having a dull metallic character, not unlike that of the protoxide of iron. When the mixture was thrown into water, there was only a very slight effervescence; but on the addition of muriatic acid to the water, globules of gas were slowly liberated, and the effect continued for nearly an hour; so that there is great reason to believe that the silex had been either entirely or partially deoxygenated, and was slowly reproduced by the action of the water, assisted by the slight attraction of the acid for the earth.

When the potassium was in the quantity of six grains, and the silex of four grains, a part of the result inflamed spontaneously as it was taken out of the tube, though the tube was quite cool, and left, as the result of its combustion, alkali and silex. The part which did not inflame, was similar in character to the matter which has been just described; it did not act upon water, but effervesced with muriatic acid.

Potassium, in acting upon alumine and glucine, produced more hydrogen than could be ascribed to the moisture present in the crust of potash; from which it seems probable that, even after ignition, water adheres to these earths.

The results of the action of the potassium were pyrophoric substances of a dark gray colour, which burnt, throwing off brilliant sparks,* and leaving behind alkali and earth, and which hissed violently when thrown upon water, decomposing it with great violence. I examined the products in two experiments, one on alumine, and one on glucine, in which naphtha was introduced into the platina tube to prevent combustion; the masses were very friable, and presented small metallic particles, which were as soft as potassium, but so small that they could not be separated so as to be more minutely examined; they melted in boiling naphtha. Either a part of the potassium must have been employed in decomposing the earths in these experiments, or it had entered into combination with them, which is unlikely, and contrary to analogy, and opposed by some experiments which will be immediately related.

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* The results of this experiment are opposed to the idea that potassium is a compound of hydrogen and potash, or its basis; for if so, it might be expected that the hydrogen would be disengaged by the attraction of the alkali for silex. In my first experiment on this combination, I operated in an apparatus connected with water, and I found that the potassium produced as much hydrogen as if it had been made to act upon water: in this case, the metal had rapidly decomposed the vapour of the water, which must have been constantly supplied.

Supposing the metals of the earths to be produced in experiments of this kind, there was great reason to expect that they might be alloyed with the common metals, as well as with potassium. Mercury was the only substance which it was safe to try in the tube of platina. In all cases in which the potassium was in excess, I obtained amalgams by introducing mercury whilst the tube was hot; but the alkaline metal gave the characters to the amalgam, and though in the case of glucine and alumine, a white matter separated during the action of very weak muriatic acid upon the amalgam, yet I could not be entirely satisfied that there was any of the metals of these earths in triple combination.

Mixtures of the earths with potassium, intensely ignited in contact with iron filings, and covered with iron filings in a clay crucible, gave much more distinct results. Whether silex, alumine, or glucine was used, there was always a fused mass in the centre of the crucible; and this mass had perfectly metallic characters. It was in all cases much whiter and harder than iron. In the instance in which silex was used, it broke under the hammer, and exhibited a crystalline texture. The alloys from alumine and glucine were imperfectly malleable. Each afforded, by solution in acids, evaporation, and treatment with reagents, oxide of iron, alkali, and notable quantities of the earths employed in the experiment.

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* The pyrophorus from alum, which I have supposed, in the last Bakerian Lecture, to be composed of potassium, sulphur, and charcoal, probably contains this substance likewise.

Though I could not procure decided evidences of the production of an amalgam from the metals of the common earths, yet I succeeded perfectly, by the same method of operating, in making amalgams of the alkaline earths.

By passing potassium through lime and magnesia, and then introducing mercury, I obtained solid amalgams, which consisted of potassium, the metal of the earth employed, and mercury.

The amalgam from magnesia was easily deprived of its potassium by the action of water. It then appeared as a solid white metallic mass which, by exposure to the air, became covered with a dry white powder, and which, when acted upon by weak muriatic acid, gave off hydrogen gas in considerable quantities, and produced a solution of magnesia.

By operations performed in this manner, there is good reason to believe it will be possible to procure quantities of the metals of the alkaline earths, sufficient for determining their nature and agencies, and the quantities of oxygen which they absorb; and by the solution of the alloys containing metals of the common earths, it seems probable that the proportions of metallic matter in these bodies may likewise be ascertained.

On an hypothesis which I have brought before the Society, namely, that the power of chemical attraction and electrical action, may be different exhibitions of the same property of matter, and that oxygen and inflammable bodies are in relations of attraction, which correspond to the function of being negative and positive respectively; it would follow that the attractions of acids for salifiable bases would be inversely, as the quantity of oxygen that they contain; and supposing the power of attraction to be measured by the quantity of basis which an acid dissolves, it would be easy to infer the quantities of oxygen and metallic matter from the quantities of acids and of basis in a neutral salt. On this idea I had, early in 1808, concluded that barytes must contain least oxygen of all the earths, and that the order, as to the quantity of inflammable matter, must be strontites, potash, soda, lime, and so on; and that silex must contain the largest quantity of oxygen of all.

If the most accurate analysis be taken, barytes may be conceived to contain about 90·5* of metal per cent., strontites 86,† lime 73·5,* magnesia 66.‡

The same proportions would follow from an application of Mr. Dalton's ingenious supposition,§ that the proportion of oxygen is the same in all protoxides, and that the quantity of acid is the same in all neutral salts, i. e., that every neutral salt is composed of one particle of metal, one of oxygen, and one of acid.

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* Mr. James Thompson, Nicholson's Journal, 1809, p. 175, and Berthier.

† Mr. Clayfield Thomson's Chemistry, vol. ii. p. 626· 629.

‡ Murray's Chemistry, vol. iii. p. 616.

§ The principle that I have stated of the affinity of an acid for a salifiable basis being inversely as the quantity of oxygen contained by the basis, though gained from the comparison of the electrical relations of the earths with their chemical affinities, in its numerical applications, must be considered merely as a consequence of Mr. Dalton's

We are in possession of no accurate experiments on the quantities of acids required to dissolve alumine, glucine, and silex; but according to Richter's estimation of the composition* of phosphate of alumine, alumine would appear to contain about 56 per cent. of metallic matter.

M. Berzelius,† in a letter which I received from him a few months ago, states that in making an analysis of cast iron, he found that it contained the metal of silex,

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law of general proportions. Mr. Dalton had, indeed, in the spring of 1808, communicated to me a series of proportions for the alkalies and alkaline earths; which, in the case of the alkalies, were not very remote from what I had ascertained by direct experiments. M. Gay Lussac's principle, that the quantity of acid in metallic salts is directly as the quantity of oxygen, might (as far as it is correct) be inferred from Mr. Dalton's law, though this ingenious chemist states, that he was led to it by different considerations. According to Mr. Dalton, there is a proportion of oxygen the same in all protoxides; and there is a proportion of acid the same in all neutral salts; and new proportions of oxygen and acid are always multiples of these proportions. So that if a protoxide in becoming a deutoxide, takes up more acid, it will be at least double the quantity, and in these cases the oxygen will be strictly as the acid. Mr. Dalton's law even provides for cases to which M. Gay Lussac's will not apply, a deutoxide may combine with a simple quantity of acid, or a protoxide with a double quantity. Thus in the insoluble oxysulphat of iron perfectly formed, (as some experiments which I have lately made, seem to shew,) there is probably only a single proportion of acid; and in the super-tartrite of potash there is only a single quantity of oxygen, and a double quantity of acid. Whether Mr. Dalton's law will apply to *all cases*, is a question which I shall not in this place attempt to discuss.

* Thomson's Chemistry, vol. ii. p. 581.

† In the same communication this able chemist informed me, that he had succeeded in decomposing the earths, by igniting them strongly with iron and charcoal.

and that this metal in being oxidated took up nearly half its weight of oxygen.

If the composition of ammonia be calculated upon, according to the principle above stated, it ought to consist of 53 metallic matter, and about 47 of oxygen,* which agrees very nearly with the quantity of hydrogen and ammonia produced from the amalgam.

Though the early chemists considered the earths and the metallic oxides as belonging to the same class of bodies, and the earths as calces, which they had not found the means of combining with phlogiston, and though Lavoisier insisted upon this analogy with his usual sagacity, yet still the alkalies, and earths, and oxides, have been generally considered as separate natural orders. The earths, it has been said, are not precipitated by the triple prussiates, or by the solutions of galls;† and the alkalies and alkaline earths, are both distinguished by their solubility in water; but if such characters be admitted as grounds of distinct classification, the common metals must be arranged under many different divisions; and the more the subject is inquired into, the more distinct will the general relations of all metallic substances appear. The alkalies and alkaline earths combine with prussic acid, and form compounds of different degrees of solubility; and solutions of barytes (as has been shewn by Dr. Henry and M. Guyton,) precipitate the triple prussiate; the power of combination is general, but the compounds formed, are soluble in different degrees in water. The case is analogous with solutions of galls; these, as I have mentioned in a paper, published in the Philosophical Transactions for 1805,* are precipitated by almost all neutro-saline solutions; and they form compounds more or less soluble in water, more or less coloured, and differently coloured with all salifiable bases. It is needless to dwell upon the combinations of the alkalies and earths, with oils, to form soaps; and of the earthy soaps, some are equally insoluble with the metallic soaps. The oxide of tin, and other oxides abounding in oxygen, approach very near in their general characters to zircon, silex and alumine; and in habits of amalgamation, and of alloy, how near do the metals of the alkalies approach to the lightest class of oxidable metals?

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* I take the proportions of the volumes from the very curious paper of M. Gay Lussac, on the combinations of gaseous bodies, Mem. d'Arcueil, tom. ii. p. 213, and the weights from my own estimation, according to which 100 cubic inches of muriatic acid gas weigh 39 grains, at the mean temperature and pressure, which is very nearly the same as the weight given by MM. Gay Lussac and Thenard.

† Klaproth. Annales de Chemie, tom. x. p. 277.

It will be unnecessary, I trust, to pursue these analogies any further, and I shall conclude this section by a few remarks on the alloys of the metals of the common earths.

It is probable that these alloys may be formed in many metallurgical operations, and that small quantities of them may influence materially the properties of the compound, in which they exist.

In the conversion of cast iron into malleable iron, by the process of blooming, a considerable quantity of glass separates, which, as far as I have been able to determine, from a coarse examination, is principally silex, alumine and lime, vitrified with oxide of iron.

Cast-iron from a particular spot will make only cold short iron; whilst, from another spot, it will make hot short; but by a combination of the two in due proportions, good iron is produced; may not this be owing to the circumstance of their containing different metals of the earths, which in compound alloy, may be more oxidable than in simple alloys, and may be more easily separated by combustion?

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* [Vol. II. p. 257.]

Copper, M. Berzelius informs me, is hardened by silicium. In some experiments that I made on the action of potassium and iron on silex, the iron, as I have mentioned before, was rendered white, and very hard and brittle, but it did not seem to be more oxidable. Researches upon this subject do not appear unworthy of pursuit, and they may possibly tend to improve some of our most important manufactures, and give new instruments to the useful arts

V. *Some considerations of Theory illustrated by new Facts.*

Hydrogen is the body which combines with the largest proportion of oxygen, and yet it forms with it a neutral compound. This, on the hypothesis of electrical energy, would shew, that it must be much more highly positive than any other substance; and, therefore, if it be an oxide, it is not likely that it should be deprived of oxygen by any simple chemical attractions. The fact of its forming a substance approaching to an acid in its nature, when combined with a metallic substance, tellurium, is opposed to the idea of its being a gaseous metal, and perhaps to the idea that it is simple, or that it exists in its common form in the amalgam of ammonium. The phenomena presented by sulphuretted hydrogen, are of the same kind, and lead to similar conclusions.

Muriatic acid gas, as I have shewn, and, as is farther proved by the researches of MM. Gay Lussac and Thenard, is a compound of a body unknown in a separate state and water. The water, I believe, cannot be decompounded, unless a new combination is formed; thus it is not changed by charcoal ignited in the gas by Voltaic electricity; but it is decompounded by all the metals; and in these cases hydrogen is elicited, in a manner similar to that in which one metal is precipitated by another; the oxygen being found in the new compound. This at first view, might be supposed in favour of the idea, that hydrogen is a simple substance; but the same reasoning may be applied to a protoxide as to a metal; and in the case of the nitro-muriatic acid, when the nitrous acid is decomposed to assist in the formation of a metallic muriate, the body disengaged (nitrous gas) is known to be in a high state of oxygenation.

That nitrogen is not a metal in the form of a gas, is almost demonstrated by the nature of the fusible substance from ammonia, and (even supposing no reference to be made to the experiments detailed in this paper) the general analogy of chemistry would lead to the notion of its being compounded.

Should it be established by future researches, that hydrogen is a protoxide of ammonium, ammonia a deutoxide, and nitrogen a tritoxide of the same metal, the theory of chemistry would attain a happy simplicity, and the existing arrangements would harmonize with all the new facts. The class of pure inflammable bases, would be *metals* capable of alloying with each other, and of combining with protoxides. Some of the bases would be known only in combination, those of sulphur, phosphorus,* and of the boracic, fluoric, and muriatic acids;

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* The electrization of sulphur and phosphorus, goes far to prove that they contain combined hydrogen. From the phenomena of the action of potassium upon them in my first experiments, I conceived that they

but the relations of their compounds would lead to the suspicion of their being metallic. The salifiable bases might be considered either as protoxides, deutoxides, or tritoxides: and the general relations of salifiable matter to acid matter, might be supposed capable of being ascertained by their relations to oxygen, or by the peculiar state of their electrical energy.

The whole tenure of the antiphlogistic doctrines, necessarily points to such an order; but in considering the facts under other points of view, solutions may be found, which if not so simple, account for the phenomena, with at least equal facility.

If hydrogen, according to an hypothesis to which I have often referred, be considered as the principle which gives inflammability, and as the cause of metallization, then our list of simple substances will include oxygen, hydrogen, and unknown bases only; metals and inflammable solids will be compounds of these bases with

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contained oxygen, though as I have stated in the appendix to the last Bakerian Lecture, the effects may be explained on a different supposition. The vividness of the ignition in the process, appeared an evidence in favour of their containing oxygen, till I discovered that similar phenomena were produced by the combination of arsenic and tellurium with potassium. In some late experiments on the action of potassium on sulphur and phosphorus, and on sulphuretted hydrogen, and on phosphuretted hydrogen, I find that the phenomena differ very much according to the circumstance of the experiment, and in some instances, I have obtained a larger volume of gas from potassium after it had been exposed to certain of these bodies, than it would have given alone. These experiments are still in progress, and I shall soon lay an account of them before the Society. The idea of the existence of oxygen in sulphur and phosphorus, is however still supported by various analogies. Their being non-conductors of electricity is one argument in favour of this. Potassium and sodium I find when heated in hydrogen, mixed with a small quantity of atmospheric air, absorb both oxygen and hydrogen, and become non-conducting inflammable bodies analogous to resinous and oily substances.

hydrogen; the earths, the fixed alkalies, metallic oxides, and the common acids, will be compounds of the same bases, with water.

The strongest arguments in favour of this notion, in addition to those I have before stated, which at present occur to me, are:—First, The properties which seem to be inherent in certain bodies, and which are either developed or concealed, according to the nature of their combinations. Thus, sulphur, when it is dissolved in water, either in combination with hydrogen or oxygen, uniformly manifests acid properties; and the same quantity of sulphur, whether in combination with hydrogen, whether in its simple form, or in combination with one proportion of oxygen, or a double proportion, from my experiments seems to combine with the same quantity of alkali. Tellurium, whether in the state of oxide or of hydruret, seems to have the same tendency of combination with alkali; and the alkaline metals, and the acidifiable bases, act with the greatest energy on each other.

Second, The facility with which metallic substances are revived, in cases in which hydrogen is present. I placed two platina wires, positively and negatively electrified from 500 double plates of 6 inches, in fused litharge; there was an effervescence at the positive side, and a black matter separated at the negative side, but no lead was produced; though when litharge, moistened with water, was employed, or a solution of lead, the metal rapidly formed; the difference of conducting power may be supposed to produce some difference of effect, yet the experiment is favourable to the idea, that the presence of hydrogen is essential to the production of the metal.

Third, Oxygen and hydrogen are bodies that in all cases seem to neutralize each other, and therefore in the products of combustion, it might be expected that the natural energies of the bases would be most distinctly displayed, which is the case; and in oxymuriatic acid, the acid energy seems to be blunted by oxygen, and is restored by the addition of hydrogen.

In the action of potassium and sodium upon ammonia, though the quantity of hydrogen evolved in my experiments is not exactly the same as that produced by their action upon water; yet it is probable, that this is caused by the imperfection of the process,* and supposing potassium and sodium to produce the same quantity of hydrogen from ammonia and water, the circumstance at first view, may be conceived favourable to the notion, that they contain hydrogen, which under common circumstances of combination, will be repellent to matter of the same kind; but this is a superficial consideration of the subject, and the conclusion cannot be admitted; for on the idea that in compounds in general, the elements are combined in uniform proportions, then whenever bodies known to contain hydrogen are decomposed by a metal, the quantities of hydrogen ought to be the same, or multiples of each other. Thus in the decomposition of ammonia by potassium and sodium, two of hydrogen and one of nitrogen remain in combination, and one of hydrogen is given off; and in the action of water on potassium to form potash, the same quantity of hydrogen ought to be expelled. From my analysis* of sulphuretted hydrogen, it would appear, that if potassium in forming a combination with this substance sets free hydrogen, it will be nearly the same quantity as it would cause to be evolved from water. And if the analysis of Mr. Proust and Mr. Hatchett, of the sulphuret of iron, be made a basis of calculation, iron, in attracting sulphur from sulphuretted hydrogen, will liberate the same proportion of hydrogen as during its solution in diluted sulphuric acid; and taking Mr. Dalton's law of proportion, the case will be similar with respect to other metals; and if such reasoning were to be adopted, as that metals are proved to be compounds of hydrogen, because in acting upon different combinations containing hydrogen, they produce the evolution of equal proportions of this gas, then it might be proved that almost any kind of matter is contained in any other. The same quantity of potash, in acting upon either muriate, sulphate, or nitrate of magnesia, will precipitate equal quantities of magnesia; but it would be absurd to infer from this, that potash contained magnesia, as one of its elements; the power of repelling one kind of matter, and of attracting another kind, must be equally definite, and governed by the same circumstances.

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* There seems to be always the same proportion between the quantity of ammonia which disappears, and the quantity of hydrogen evolved; *i.e.* whenever the metals of the alkalies act upon ammonia (supposing this body to be composed of 3 of hydrogen and 1 of nitrogen in volume,) 2 of hydrogen and 1 of nitrogen remain in combination, and 1 of hydrogen is set free. And it may be adduced as a strong argument in favour of the theory of definite proportions, that the quantity of the metals of the alkalies and nitrogen, in the fusible results, are in the same proportions as those in which they exist in the alkaline nitrats.

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* The composition may be deduced from the experiments in the last Bakerian Lecture, which shew that it contains a volume of hydrogen equal to its own. If its specific gravity be taken as 35 grains, for 100 cubical inches, then it will consist of 2·27 of hydrogen, and 32·73 of sulphur. When sulphuretted hydrogen is decomposed by common electricity, in very refined experiments, there is a slight diminution of volume, and the precipitated sulphur has a whitish tint, and probably contains a minute quantity of hydrogen. When it is decomposed by Voltaic sparks, the sulphur is precipitated in its common form, and there is no change of volume; in the last case the sulphur is probably ignited at the moment of its production. In some experiments lately made in the laboratory of the Royal Institution, on arseniuretted, and phosphuretted hydrogen, it was found that when these gases were decomposed by electricity, there was no change in their volumes; but neither the arsenic nor the phosphorus seemed to be thrown down in their common states; the phosphorus was dark coloured, and the arsenic appeared as a brown powder, both were probably hydrurets; this is confirmed likewise by the action of potassium upon arseniuretted and phosphuretted hydrogen; when the metal is in smaller quantity than is sufficient to decompose the whole of the gases, there is always an expansion of volume; so that arseniuretted and phosphuretted hydrogen contain in equal volumes, more hydrogen than sulphurretted hydrogen, probably half as much more, or twice as much more. From some experiments made on the weights of phosphuretted and arseniuretted hydrogen, it would appear, that 100 cubic inches of the first weigh about 10 grains, at the mean temperature and pressure, and 100 of the second about 15 grains.

Potassium, sodium, iron, mercury, and all metals that I have experimented upon, in acting upon muriatic acid gas, evolve the same quantity of hydrogen, and all form dry muriates; so that any theory of metallization applicable to potash and soda, must likewise apply to the common metallic oxides. If we assume the existence of water in the potash, formed in muriatic acid gas, we must likewise infer its existence in the oxides of iron and mercury, produced in similar operations.

The solution of the general question concerning the presence of hydrogen in all inflammable bodies, will undoubtedly be influenced by the decision upon the nature of the amalgam from ammonia, and a matter of so much importance ought not to be hastily decided upon. The difficulty of finding any multiple of the quantity of oxygen, which may be supposed to exist in hydrogen, that might be applied to explain the composition of nitrogen from the same basis, is undoubtedly against the simplest view of the subject. But still the phlogistic explanation, that the metal of ammonia is merely a compound of hydrogen and nitrogen; or that a substance which is metallic, can be composed from substances not in their own nature metallic, is equally opposed to the general tenour of our chemical reasonings.

I shall not at present occupy the time of the Society by entering any further into these discussions; hypotheses can scarcely be considered as of any value, excepting as leading to new experiments; and the objects in the novel field of electro-chemical research, have not been sufficiently examined to enable me to decide upon their nature and their relations, or to form any general theory concerning them which is likely to be permanent.

EXPLANATION OF THE FIGURES.

Fig. 1. The apparatus for electrizing potassium in gases. A, the glass tube; B, the wire negatively electrified; C and D, the cup and wire positively electrified.

Fig. 2. The apparatus for decomposing water out of the contact of air, page 250. AA, the cones containing the water; BBB, the tubes for conveying the gas. C and D, the pneumatic apparatus.

Fig. 3. The apparatus for decomposing and recomposing water under oil. CC, the wires for communicating the Voltaic electricity; DD, the wires for producing the explosion; B, the tube; A, the vessel containing it. *a d c*, the level of the different fluids.

Fig. 4. The apparatus for exposing water to the action of ignited potash and charcoal out of the contact of air. A, the tube for water; B, the iron tube; C, the receiver for ammonia; D, the pneumatic apparatus.

Fig. 5. The apparatus for the decomposition of ammonia.

Fig. 6. A Voltaic apparatus, being one of the 200

PLATE 3.

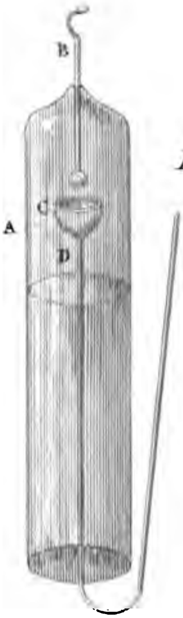


Fig 1

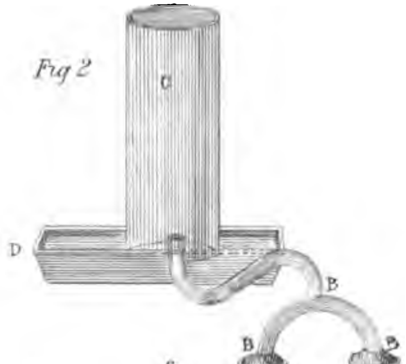


Fig 2

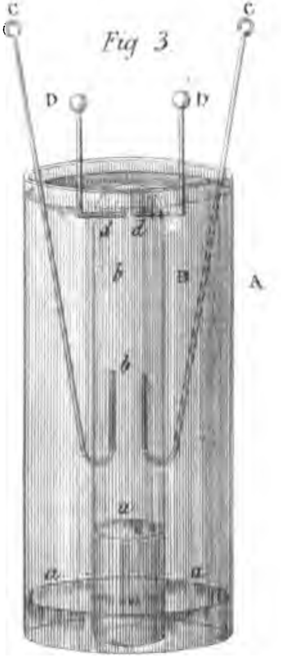
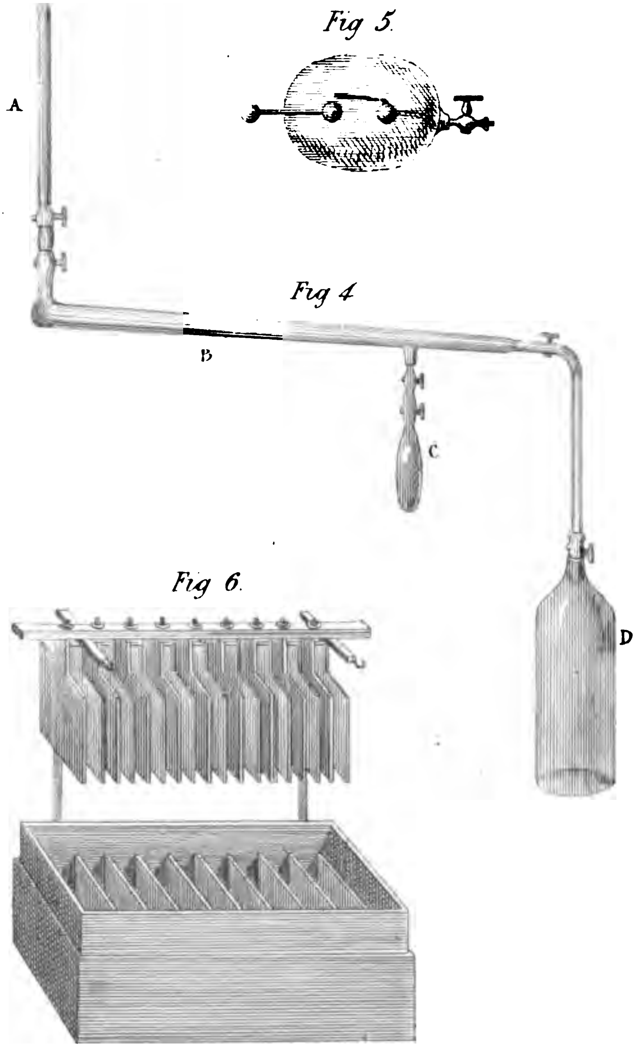


Fig 3

PLATE 4.



which compose the new Voltaic battery of the Royal Institution. For the construction of this battery, and of other instruments applicable to new researches, a fund of upwards of 1000*l.* has been raised by subscription from members of the Royal Institution. As yet the whole combination has not been put into action; but reasoning from the effects of that part of it which has been used, some important phenomena may be expected from so great an accumulation of electrical power.

VII.

RESEARCHES ON THE OXYMURIATIC ACID, ITS NATURE AND COMBINATIONS; AND ON THE ELEMENTS OF THE MURIATIC ACID: WITH SOME EXPERIMENTS ON SULPHUR AND PHOSPHORUS, MADE IN THE LABORATORY OF THE ROYAL INSTITUTION.*

THE illustrious discoverer of the oxymuriatic acid considered it as muriatic acid freed from hydrogen;† and the common muriatic acid as a compound of hydrogen and oxymuriatic acid; and on this theory he denominated oxymuriatic acid dephlogisticated muriatic acid.

M. Berthollet,‡ a few years after the discovery of Scheele, made a number of important and curious experiments on this body; from which he concluded that it was composed of muriatic acid gas and oxygen; and this idea for nearly twenty years has been almost universally adopted.

Dr. Henry, in an elaborate series of experiments, made with the view of decomposing muriatic acid gas, ascertained that hydrogen was produced from it by electricity; and he attributed the phenomena to water contained in the gas.§

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* [From Phil. Trans. for 1810. Read before the Royal Society, July 12th, 1810.]

† Mem. Acad. Stockholm, for 1774, p. 94.

‡ Journal de Physique, 1785, p. 325.

§ Phil. Trans. for 1800, p. 191.

In the Bakerian Lecture for 1808, I have given an account of the action of potassium upon muriatic acid gas, by which more than one-third of its volume of hydrogen is produced; and I have stated, that muriatic acid can in no instance be procured from oxymuriatic acid, or from dry muriates, unless water or its elements be present.

In the second volume of the Memoires d'Arcueil, MM. Gay Lussac and Thenard have detailed an extensive series of facts upon muriatic acid, and oxymuriatic acid. Some of their experiments are similar to those I have detailed in the paper just referred to; others are peculiarly their own, and of a very curious kind: their general conclusion is, that muriatic acid gas contains about one quarter of its weight of water; and that oxymuriatic acid is not decomposable by any substances but hydrogen, or such as can form triple combinations with it.

One of the most singular facts that I have observed on this subject, and which I have before referred to, is, that charcoal, even when ignited to whiteness in oxymuriatic or muriatic acid gases, by the Voltaic battery, effects no change in them; if it has been previously freed from hydrogen and moisture by intense ignition in vacuo.

This experiment, which I have several times repeated, led me to doubt of the existence of oxygen in that substance, which has been supposed to contain it above all others in a loose and active state; and to make a more vigorous investigation than had been hitherto attempted for its detection.

If oxymuriatic acid gas be introduced into a vessel exhausted of air, containing tin, and the tin be gently heated, and the gas in sufficient quantity, the tin and the gas disappear, and a limpid fluid, precisely the same as Libavius's liquor is formed;—it occurred to me, that if this substance is a combination of muriatic acid and oxide of tin, oxide of tin ought to be separated from it by means of ammonia. I admitted ammoniacal gas over mercury to a small quantity of the liquor of Libavius; it was absorbed with great heat, and no gas was generated; a solid result was obtained, which was of a dull white colour; some of it was heated, to ascertain if it contained oxide of tin; but the whole volatilized, producing dense pungent fumes.

Another experiment of the same kind, made with great care, and in which ammonia was used in great excess, proved that the liquor of Libavius cannot be decompounded by ammonia; but that it forms a new combination with this substance.

I have described, on a former occasion, the nature of the operation of phosphorus on oxymuriatic acid, and I have stated that two compounds, one fluid and the other solid, are formed in the process of combustion, of which the first, on the generally received theory of the nature of oxymuriatic acid, must be considered as a compound of muriatic acid and phosphoric acid. It occurred to me, that if the acids of phosphorus really existed in these combinations, it would not be difficult to obtain them, and thus to gain proof of the existence of oxygen in oxymuriatic acid.

I made a considerable quantity of the solid compound of oxymuriatic and phosphorus by combustion, and saturated it with ammonia by heating it in a proper receiver filled with ammoniacal gas, on which it acted with great energy, producing much heat; and they formed a white opaque powder. Supposing that this substance was composed of the dry muriates and phosphates of ammonia; as muriate of ammonia is very volatile, and as ammonia is driven off from phosphoric acid, by a heat below redness, I conceived that by igniting the product obtained, I should procure phosphoric acid; I therefore introduced some of the powder into a tube of green glass, and heated it to redness, out of the contact of air by a spirit lamp; but found, to my great surprise, that it was not at all volatile nor decomposable at this degree of heat, and that it gave off no gaseous matter.

The circumstance that a substance composed principally of oxymuriatic acid, and ammonia, should resist decomposition or change at so high a temperature, induced me to pay particular attention to the properties of this new body.

It had no taste nor smell; it did not seem to be soluble, nor did it undergo any perceptible change when digested in boiling water; it did not appear to be acted upon by sulphuric, muriatic, or nitric acids, nor by a strong lixivium of potash. The only processes by which it seemed susceptible of decomposition were by combustion, or the action of ignited hydrat of potash. When brought into the flame of a spirit lamp and made red-hot, it gave feeble indications of inflammation, and tinged the flame of a yellow colour, and left a fixed acid, having the properties of phosphoric acid. When acted on by red-hot hydrat of potash, it emitted a smell of ammonia, burnt where it was in contact with air, and appeared to dissolve in the alkali. The potash which had been so acted upon gave muriatic acid, by the addition of sulphuric acid.

I heated some of the powder to whiteness, in a tube of platina; but it did not appear to alter; and after ignition gave ammonia by the action of fused hydrat of potash.

I caused ammonia, made as dry as possible, to act on the phosphuretted liquor of MM. Gay Lussac and Thenard; and on the sulphuretted muriatic liquor of Dr. Thomson; but no decomposition took place; nor was any muriate of ammonia formed, when proper precautions were taken to exclude moisture. The results were new combinations; that from the phosphuretted liquor was a white solid, from which a part of the phosphorus was separated by heat; but which seemed no further decomposable, even by ignition. That from the sulphuretted liquor was likewise solid, and had various shades of colour, from a bright purple to a golden yellow, according as it was more or less saturated with ammonia: but as these compounds did not present the same uniform and interesting 'properties, as that from the phosphoric sublimate, I did not examine them minutely: I contented myself by ascertaining that no substance known to contain oxygen could be procured from oxymuriatic acid, in this mode of operation.

It has been said, and taken for granted by many chemists, that when oxymuriatic acid and ammonia act upon each other, water is formed; I have several times made the experiment, and I am convinced that this is not the case. When about 15 or 16 parts of oxymuriatic acid gas are mixed with from 40 to 45 parts of ammoniacal gas, there is a condensation of nearly the whole of the acid and alkaline gases, and from 5 to 6 parts of nitrogen are produced; and the result is dry muriate of ammonia.

Mr. Cruikshank has shown that oxymuriatic acid and hydrogen, when mixed in proportions nearly equal, produce a matter almost entirely condensible by water;*

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* [Nicholson's Journal, 4to, vol. v. p. 206. The result of Mr. Cruikshank's experiments on exploding together by means of the electric spark, mixtures of hydrogen and chlorine, was, "that three measures of

and MM. Gay Lussac and Thenard, have stated, that this matter is common muriatic acid gas, and that no water is deposited in the operation. I have made a number of experiments on the action of oxymuriatic acid gas and hydrogen. When these bodies were mixed in equal volumes over water, and introduced into an exhausted vessel and fired by the electric spark, there was always a deposition of a slight vapour, and a condensation of from $\frac{1}{10}$ to $\frac{1}{30}$ of the volume; but the gas remaining was muriatic acid gas. I have attempted to make the experiment in a manner still more refined, by drying the oxymuriatic acid and the hydrogen by introducing them into vessels containing muriate of lime, and by suffering them to combine at common temperatures; but I have never been able to avoid a slight condensation; though in proportion as the gases were free from oxygen or water, this condensation diminished.

I mixed together sulphuretted hydrogen in a high degree of purity and oxymuriatic acid gas both dried, in equal volumes: in this instance, the condensation was not $\frac{1}{40}$; sulphur, which seemed to contain a little oxymuriatic acid, was formed on the sides of the vessel; no vapour was deposited; and the residual gas contained about $\frac{1}{10}$ of muriatic acid gas, and the remainder was inflammable.

MM. Gay Lussac and Thenard have proved by a copious collection of instances, that in the usual cases where oxygen is procured from oxymuriatic acid, water is always present, and muriatic acid gas is formed; now, as it is shewn that oxymuriatic acid gas is converted into muriatic acid gas by combining with hydrogen, it is scarcely possible to avoid the conclusion, that the oxygen

hydrogenous gas requires three and a half of oxygenated muriatic acid gas to saturate them; the products being water and muriatic acid."]

is derived from the decomposition of the water, and, consequently, that the idea of the existence of water in muriatic acid gas, is hypothetical, depending upon an assumption which has not yet been proved—the existence of oxygen in oxymuriatic acid gas.

MM. Gay Lussac and Thenard indeed have stated an experiment, which they consider as proving that muriatic acid gas contains one quarter of its weight of combined water. They passed this gas over litharge, and obtained so much water; but it is obvious, that in this case, they formed the same compound as that produced by the action of oxymuriatic acid on lead; and in this process the muriatic acid must lose its hydrogen and the lead its oxygen; which of course would form water; these able chemists, indeed, from the conclusion of their memoir, seem aware, that such an explanation may be given, for they say, that the oxymuriatic acid *may* be considered as a simple body.

I have repeated those experiments which led me first to suspect the existence of combined water in muriatic acid with considerable care; I find that, when mercury is made to act upon one in volume of muriatic acid gas, by Voltaic electricity, all the acid disappears, calomel is formed, and about ·5 of hydrogen evolved.

With potassium, in experiments made over dry mercury, the quantity of hydrogen is always from 9 to 11, the whole of the muriatic acid gas used being 20. And in some experiments made very carefully by my brother, Mr. John Davy, on the decomposition of muriatic acid gas, by heated tin and zinc, hydrogen, equal to about half its volume, was disengaged, and metallic muriates, the same as those produced by the combustion of tin and zinc in oxymuriatic gas, resulted.

It is evident from this series of observations, that

Scheele's view (though obscured by terms derived from a vague and unfounded general theory) of the nature of the oxymuriatic and muriatic acids, may be considered as an expression of facts; whilst the view adopted by the French school of chemistry, and which, till it is minutely examined, appears so beautiful and satisfactory, rests in the present state of our knowledge upon hypothetical grounds. When oxymuriatic acid is acted upon by nearly an equal volume of hydrogen, a combination takes place between them, and muriatic acid gas results. When muriatic acid gas is acted upon by mercury, or any other metal, the oxymuriatic acid is attracted from the hydrogen by the stronger affinity of the metal; and an oxymuriate exactly similar to that formed by combustion is produced.

The action of water upon these compounds, which have been usually considered as muriates, or as dry muriates, but which are properly combinations of oxymuriatic acid with inflammable bases, may be easily explained according to these views of the subject. When water is added in certain quantities to Libavius's liquor, a solid crystallized mass is obtained, from which oxide of tin and muriate of ammonia can be procured. In this case, oxygen may be conceived to be supplied to the tin, and hydrogen to the oxymuriatic acid.

The compound formed by burning phosphorus in oxymuriatic acid, is in a similar relation to water: if that substance be added to it, it is resolved into two powerful acids: oxygen it may be supposed is furnished to the phosphorus to form phosphoric acid, hydrogen to the oxymuriatic acid to form common muriatic acid gas.

None of the combinations of the oxymuriatic acid with inflammable bodies, can be decomposed by dry acids; and this seems to be the test which distinguishes the oxymuriatic combinations from the muriates, though they have hitherto been confounded together. Muriate of potash, for instance, if M. Berthollet's estimation of its composition approaches towards accuracy, when ignited, is a compound of oxymuriatic acid with potassium; muriate of ammonia is a compound of muriatic acid gas and ammonia; and when acted on by potassium, it is decompounded; the oxymuriatic acid may be conceived to combine with the potassium to form muriate of potash, and the ammonia and hydrogen are set free.

The vivid combustion of bodies in oxymuriatic acid gas, at first view, appears a reason why oxygen should be admitted in it; but heat and light are merely results of the intense agency of combination. Sulphur and metals, alkaline earths and acids become ignited during their mutual agency; and such an effect might be expected in an operation so rapid as that of oxymuriatic acid upon metals and inflammable bodies.

It may be said, that a strong argument in favour of the hypothesis, that oxymuriatic acid consists of an acid basis united to oxygen, exists in the general analogy of the compounds of oxymuriatic acid and metals, to the common neutral salts; but this analogy when strictly investigated, will be found to be very indistinct; and even allowing it, it may be applied with as much force to support an opposite doctrine, namely, that the neutral salts are compounds of bases with water; and the metals of bases with hydrogen; and that in the case of the action of oxymuriatic acid and metals, the metal furnishes hydrogen to form muriatic acid, and a basis to produce the neutral combination.

That the quantity of hydrogen evolved during the decomposition of muriatic acid gas by metals, is the same that would be produced during the decomposition of water by the same bodies, appears, at first view, an evidence in favour of the existence of water in muriatic acid gas; but as there is only one known combination of hydrogen with oxymuriatic acid, one quantity must always be separated. Hydrogen is disengaged from its oxymuriatic combination by a metal, in the same manner as one metal is disengaged by another from similar combinations; and of all inflammable bodies that form compounds of this kind, except perhaps phosphorus and sulphur, hydrogen is that which seems to adhere to oxymuriatic acid with the least force.

I have caused strong explosions from an electrical jar, to pass through oxymuriatic gas, by means of points of platina, for several hours in succession; but it seemed not to undergo the slightest change.

I electrized the oxymuriates of phosphorus and sulphur for some hours, by the power of the Voltaic apparatus of 1000 double plates; no gas separated, but a minute quantity of hydrogen, which I am inclined to attribute to the presence of moisture in the apparatus employed; for I once obtained hydrogen from Libavius's liquor by a similar operation; but I have ascertained, that this was owing to the decomposition of water, adhering to the mercury; and in some late experiments made with 2000 double plates, in which the discharge was from platina wires, and in which the mercury used for confining the liquor was carefully boiled; there was no production of any permanent elastic matter.

As there are no experimental evidences of the existence of oxygen in oxymuriatic acid gas, a natural question arises, concerning the nature of those compounds, in which the muriatic acid has been supposed to exist, combined with much more oxygen than oxymuriatic acid, in the state in which it has been named by Mr. Chenevix, hyper-oxygenized muriatic acid.

Can the oxymuriatic *acid* combine either with oxygen or hydrogen, and form with each of them an acid compound; of which that with hydrogen has the strongest, and that with oxygen the weakest affinity for bases? for the able chemist to whom I have just referred, conceives that hyper-oxymuriates are decomposed by muriatic acid. Or, is hyper-oxymuriatic acid the basis of all of this class of bodies, the most simple form of this species of matter?

The phenomena of the composition and decomposition of the hyper-oxymuriates, may be explained on either of these suppositions; but they are mere suppositions unsupported by experiment.

I have endeavoured to obtain the neutralizing acid, which has been imagined to be hyper-oxygenized, from the hyper-oxymuriate of potash, by various modes, but uniformly without success.* By distilling the salt with dry boracic acid, though a little oxymuriatic acid is generated, yet oxygen is the chief gaseous product, and a muriate of potash not decomposable is produced.

The distillation of the orange-coloured fluid, produced by dissolving hyper-oxymuriate of potash in sulphuric acid, affords only oxygen in great excess, and oxymuriatic acid.

When solutions of muriates, or muriatic acid are electrized in the Voltaic circuit, oxymuriatic acid is evolved at the positive surface, and hydrogen at the negative surface. When a solution of oxymuriatic acid in water is electrized, oxymuriatic acid and oxygen appear * at the positive surface, and hydrogen at the negative surface, facts which are certainly unfavourable to the idea of the existence of hyper-oxygenized muriatic acid, whether it be imagined a compound of oxymuriatic acid with oxygen, or the basis of oxymuriatic acid.

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* [The author afterwards succeeded in obtaining this acid; it will be found described in a subsequent paper; it has been called chloric acid, (a name proposed for it by M. Gay Lussac, who procured it, though not quite pure by a different method,) and its compounds, the old hyper-oxymuriates, chlorates.]

If the facts respecting the hyper-oxymuriate of potash, indeed, be closely reasoned upon, it must be regarded as nothing more than a triple compound of oxymuriatic acid, potassium and oxygen. We have no right to assume the existence of any peculiar acid in it, or of a considerable portion of combined water; and it is perhaps more conformable to the analogy of chemistry, to suppose the large quantity of oxygen combined with the potassium, which we know has an intense affinity for oxygen, and which from some experiments, I am inclined to believe, is capable of combining directly with more oxygen than exists in potash, than with the oxymuriatic acid, which, as far as is known, has no affinity for that substance.

It is generally supposed that a mixture of oxymuriatic acid and hyper-oxymuriatic acid is disengaged when hyper-oxymuriate of potash is decomposed by common muriatic acid,† but I am satisfied from several trials, that the gas procured in this way, when not mixed with

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* The quantity of oxymuriatic acid in the aqueous solution is so small, that the principal products must be referred to the decomposition of water. This happens in other instances, the water only is decomposed in dilute solutions of nitric and sulphuric acids.

† If hyper-oxymuriate of potash be decomposed by nitric or sulphuric acid, it affords oxymuriatic acid and oxygen. If it be acted upon by muriatic acid, it affords a large quantity of oxymuriatic acid gas only.

oxygen, unites to the same quantity of hydrogen,* as common oxymuriatic acid gas from magnesia; and I find, by a careful examination, that the gas disengaged during the solution of platina, in a mixture of nitric and muriatic acids, which has been regarded as hyper-oxymuriatic acid, but which I stated some years ago to possess the properties of oxymuriatic acid gas,† is actually that body, owing its peculiar colour to a small quantity of nitro-muriatic vapour suspended in it, and from which it is easily freed by washing.

Few substances, perhaps, have less claim to be considered as acid, than oxymuriatic acid. As yet we have no right to say that it has been decompounded; and as its tendency of combination is with pure inflammable matters, it may possibly belong to the same class of bodies as oxygen.

In this last case, the phenomena seems merely to depend upon the decomposition of the muriatic acid gas, by the oxygen loosely combined in the salt. [Vide the paper which follows next for a correction of this statement.]

* This likewise appears from Mr. Cruikshank's experiments. See Nicholson's Journal, vol. v. 4to, p. 206.

† The platina, I find by several experiments, made with great care, has no share in producing the evolution of this gas. It is formed during the production of aqua regia. The hydrogen of the muriatic acid attracts oxygen from the nitric acid. Oxymuriatic acid is set free, and nitrous gas remains in the solution, and gives it a deep red colour. *Nitrous* acid and muriatic acid produce no oxymuriatic acid gas. Platina, during its solution, in perfectly formed aqua regia, gives only nitrous gas and nitrous vapour; and I find, that rather more oxymuriatic acid gas is produced, by heating together equal quantities of nitric acid of 1·45, and muriatic acid of 1·18, when they are not in contact with platina, than when exposed to that metal. The oxymuriatic acid gas, produced from muriatic acid by nitric acid, I find combines with about an equal volume of hydrogen by detonation. [For farther information on this subject, vide a later paper by the auther, in this volume, expressly concerning it.]

May it not in fact be a *peculiar* acidifying and dissolving principle, forming compounds with combustible bodies, analogous to acids containing oxygen, or oxides, in their properties and powers of combination; but differing from them, in being for the most part decomposable by water? On this idea muriatic acid may be considered as having hydrogen for its basis, and oxymuriatic acid for its acidifying principle. And the phosphoric sublimate as having phosphorus for its basis, and oxymuriatic acid for its acidifying matter. And Libavius's liquor, and the compounds of arsenic with oxymuriatic acid, may be regarded as analogous bodies. The combinations of oxymuriatic acid with lead, silver, mercury, potassium and sodium, in this view would be considered as a class of bodies related more to oxides than acids in their powers of attraction.

It is needless to take up the time of this learned society, by dwelling upon the imperfection of the modern nomenclature of these substances. It is in many cases connected with false ideas of their nature and composition, and in a more advanced state of the inquiry, it will be necessary for the progress of science, that it should undergo material alterations.

It is extremely probable that there are many combinations of oxymuriatic acid with inflammable bodies which have not yet been investigated. With phosphorus it seems capable of combining in at least three proportions; the phosphuretted muriatic acid of Gay Lussac and Thenard, is the compound containing the maximum of phosphorus. The crystalline phosphoric sublimate, and the liquor formed by the combustion of phosphorus in oxymuriatic acid gas, disengage no phosphorus by the action of water; the sublimate, as I have already mentioned, affords phosphoric and muriatic acid; and the liquid, I believe, only phosphorous acid and muriatic acid.

The sublimate from the boracic basis gives, I believe, only boracic and muriatic acid, and may be regarded as boracium acidified by oxymuriatic acid.

It is evident, that whenever an oxymuriatic combination is decomposed by water, the oxide, or acid, or alkali, or oxidated body formed must be in the same proportion as the muriatic acid gas, as the oxygen and hydrogen must bear the same relation to each other; and experiments upon these compounds will probably afford simple modes of ascertaining the proportions of the elements, in the different oxides, acids, and alkaline earths.

If, according to the ingenious idea of Mr. Dalton, hydrogen be considered as 1 in weight, in the proportion it exists in water, then oxygen will be nearly 7·5; and assuming that potash is composed of 1 proportion of oxygen, and 1 of potassium, then potash will be 48, and potassium* about 40·5; and from an experiment which I have detailed in the last Bakerian Lecture, on the combustion of potassium in muriatic acid gas, oxymuriatic acid will be represented by 32·9, and muriatic acid gas, of course, by 33·9; and this estimate agrees with the specific gravity of oxymuriatic acid gas and muriatic acid gas. From my experiments 100 cubical inches of oxymuriatic acid gas weigh, the reductions being made for the mean temperature and pressure, 74·5 grains;† whereas by estimation they should weigh 74·6. Muriatic acid gas I find weighs, under like circumstances, in the quantity of 100 cubic inches, 39 grains; by estimation it should weigh 38·4 grains.

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* Supposing potash to contain nearly 15·6 per cent. of oxygen.

† [The author in his paper on euchlorine, states the weight of pure chlorine to be to that of common air as 244 to 100;—the gas, the weight of which is assigned in the text, contained, he afterwards inferred, from the manner in which it was prepared, a little of the heavier euchlorine.]

It is easy from these data, knowing the composition of any dry muriate, to ascertain the quantity of oxide, or of acid it would furnish by the action of water, and consequently the quantity of oxygen with which the inflammable matter will combine.*

In considering the dry muriates, as compounds of oxymuriatic acid and inflammable bodies; the argument that I have used in the last Bakerian Lecture, to shew that potassium does not form hydrate of potash by combustion, is considerably strengthened; for from the quantity of oxymuriatic acid the metal requires to produce a muriate, it seems to be shewn, that it is the simplest known form of the alkaline matter. This I think approaches to an experimentum crucis. Potash made by alcohol, and that has been heated to redness, appears to be an hydrat of potash, whilst the potash formed by the combustion of potassium must be considered as a pure metallic oxide, which requires about 19 per cent. of water to convert into a hydrat.

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* I have stated in the last Bakerian Lecture, that during the decomposition of the amalgam from ammonia, 1 in volume of hydrogen to 2 of ammonia, is evolved; it is remarkable, that whatever theory of the nature of this extraordinary compound be adopted, there will be a happy coincidence as to definite proportions. If it be supposed that the hydrogen arises from the decomposition of water; then the oxygen that must be assumed to exist in ammonia, will be exactly sufficient to neutralize the hydrogen, in an equal volume of muriatic acid; or if it be said that ammonium is a compound of 2 of ammonia and 1 of hydrogen in volume, then equal volumes of muriatic acid gas and ammonia will produce the same compound as oxymuriatic acid and ammonium, supposing they could be immediately combined. I once thought that the phenomena of metallization might be explained according to a modified phlogistic theory, by supposing three different classes of metallic bodies: First, The metal of ammonia, in which hydrogen was so loosely combined as to be separable with great ease, and which, in consequence of the small affinity of the basis for water, had little tendency to combine with oxygen. The second, the metals of the alkalies and alkaline earths, in which the hydrogen was more firmly combined, but in combustion, forming water capable of being separated from the basis. And, thirdly, the metals of the earths and common metals, in which the hydrogen was more intimately combined; producing by union with oxygen, water not separable by any new attractions. The phenomena of the action of potassium and sodium upon muriatic acid referred to in the text, seem however to overturn these speculations so far as they concern the metals from the fixed alkalies.

Amongst all the known combustible bodies, charcoal is the only one which does not combine directly with oxymuriatic acid gas;* and yet there is reason for believing that this combination may be formed by the intermedium of hydrogen. I am inclined to consider the oily substance produced by the action of oxymuriatic acid gas and olefiant gas,† as a ternary compound of these bodies; for they combine nearly in equal volumes; and I find that, by the action of potassium upon the oil so produced, muriate of potash is formed, and gaseous matter, which I have not yet been able to collect in sufficient quantity to decide upon its nature, is formed. Artificial camphor,‡ and muriatic ether,§ as is probable from the ingenious experiments of M. Gehlen and M. Thenard, must be combinations of a similar kind, one probably with more hydrogen, and the other with more carbon.

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* [Four different combinations of chlorine and carbon indirectly formed have since been obtained.]

† [This compound is now commonly considered as a binary ore, consisting of one proportion of chlorine and of one of olefiant gas.]

‡ [Artificial camphor has been found to consist of one proportion of essence of camphor (camphine) and of one of muriatic acid.]

§ [Muriatic ether appears to be composed of muriatic acid and etherine; of course, where several elements are concerned, as in these compounds, different hypothetical views may be taken of the mode of their arrangement.]

One of the greatest problems in economical chemistry is the decomposition of the muriates of soda and potash. The solution of this problem will, perhaps, be facilitated by these new views. The affinity of potassium and sodium for oxymuriatic acid is very strong; but so likewise is their attraction for oxygen, and the affinity of their oxides for water. The affinities of oxymuriatic acid gas for hydrogen, and of muriatic acid gas for water, are likewise of a powerful kind. Water, therefore, should be present in all cases when it is intended to attempt to produce alkali. It is not difficult after these views, to explain the decomposition of common salt by aluminous or siliceous substances which, as it has been long known, act only when they contain water. In these cases, the sodium may be conceived to combine with the oxygen of the water and with the earth to form a vitreous compound; and the oxymuriatic acid to unite with the hydrogen of the water, forming muriatic acid gas.

It is also easy, according to these new ideas, to explain the decomposition of salt by moistened litharge, the theory of which has so much perplexed the most acute chemists. It may be conceived to be an instance of compound affinity; the oxymuriatic acid is attracted by the lead, and the sodium combines with the oxygen of the litharge and with water to form hydrat of soda, which gradually attracts carbonic acid from the air.

As iron has a strong affinity for oxymuriatic acid, I attempted to procure soda by passing steam over a mixture of iron filings and muriate of soda intensely heated: and in this way I succeeded in decomposing some of the salt; hydrogen came over, a little hydrate of soda was formed, and muriate of iron was produced.

It does not seem improbable, supposing the views that have been developed accurate, that by complex affinities, even potassium and sodium in their metallic forms, may be procured from their oxymuriatic combinations: for this purpose the oxymuriatic acid should be attracted by one substance, and the alkaline metals by another, and such bodies should be selected for the experiments as would produce compounds differing considerably in degree of volatility.*

I cannot conclude the subject of the application of these doctrines, without asking permission to direct the attention of the society, to some of the theoretical relations of the facts noticed in the preceding pages.

That a body principally composed of oxymuriatic acid and ammonia, two substances which have been generally conceived incapable of existing together, should be so difficult of decomposition, as to be scarcely affected by any of the agents of chemistry, is a phenomenon of a perfectly new kind. Three bodies, two of which are permanent gases, and the other of which is considered volatile, form in this instance a substance neither fusible nor volatile, at a white heat. It could not have been expected that ammonia would remain fixed at such a temperature; but that it should remain fixed in combination with oxymuriatic acid, would have appeared incredible, according to all the existing analogies of chemistry. The experiments in which these conclusions are founded, are, however, uniform in their results;

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* [The above idea of the author has subsequently been amply verified; he himself obtained sodium from common salt, by the action of potassium, and the most successful method of procuring the metallic bases of certain of the earths has been found to be by the decomposition of their chlorides by the same metal.]

and it is easy to repeat them. They seem to shew, that the common chemical proposition, that complexity of composition is uniformly connected with facility of decomposition, is not well founded. The compound of oxymuriatic acid, phosphorus, and ammonia, resembles an oxide, such as silex, or that of columbium in its general chemical characters, and is as refractory when treated by common reagents; and except by the effects of combustion, or the agency of fused potash, its nature could not be detected by any of the usual methods of analysis. Is it not likely, reasoning from these circumstances, that many of the substances, now supposed to be elementary may be reduced into simpler forms of matter? And that an intense attraction, and an equilibrium of attraction, may give to a compound, containing several constituents, that refractory character, which is generally attributed to unity of constitution, or to the homogeneous nature of its parts?

Besides the compound of the phosphoric sublimate and ammonia, and the other analogous compounds which have been referred to, it is probable that other compounds of like nature may be formed of the oxides, alkalies, and earths, with the oxymuriatic combinations, or of the oxymuriatic compounds with each other: and should this be the case, the more refined analogies of chemical philosophy will be extended by these new, and as it would seem at first view, contradictory facts. For if, as I have said, oxymuriatic acid gas be referred to the same class of bodies as oxygen gas, then, as oxygen is not an acid, but forms acids by combining with certain inflammable bodies, so oxymuriatic acid, by uniting to similar substances, may be conceived to form either acids, which is the case when it combines with hydrogen, or compounds like acids or oxides, capable of forming neutral combinations, as in the instances of the oxymuriates of phosphorus and tin.

Like oxygen, oxymuriatic acid is attracted by the positive surface in Voltaic combinations; and on the hypothesis of the connection of chemical attraction with electrical powers, all its energies of combination correspond with those of a body supposed to be negative in a high degree.

And in most of its compounds, except those containing the alkaline metals, which may be conceived in the highest degree positive, and the metals with which it forms insoluble compounds, it seems still to retain its negative character.

I shall occupy the time of the society for a few minutes longer only, for the purpose of detailing a few observations connected with the Bakerian Lectures, delivered in the two last years; particularly those parts of them relating to sulphur and phosphorus, which new and more minute inquiries have enabled me to correct or extend.

I have already mentioned, that there are considerable differences in the results of experiments made on the action of potassium, on sulphur and phosphorus, and their combinations with hydrogen, according to different circumstances of the process. I shall now refer to such of these circumstances as I have been able fully to investigate.

The able researches of Dr. Thomson have shewn that sulphur, in its usual state, contains small quantities of acid matter; and though in my first experiments, I conceived that by employing crystallized native sulphur, which had been recently sublimed in nitrogen, I should avoid the presence of any foreign matter, yet I am inclined to believe that this is not the case; for by subliming some similar sulphur in nitrogen, I find that litmus paper placed in the upper part of the retort is slightly reddened.

When potassium is made to unite with sulphur, if the retort employed is not lined with sulphur, some of the potassium is destroyed by acting upon the glass; and when large quantities of sulphur are used, it is very difficult to decompose the whole of the sulphuret of potassium by an acid: sulphuretted hydrogen likewise is soluble in muriatic acid; and this circumstance led me to underrate the quantity of sulphuretted hydrogen given off in experiments of this kind.*

In acting upon sulphuretted hydrogen by potassium in my early experiments, I used large quantities of the gas and of the metal; and in these cases I have reason to believe that the violence of the combustion, occasioned the decomposition of a considerable quantity of the gas; and, in consequence, led me to form erroneous conclusions concerning the nature of this curious operation.

In all late experiments in which sulphur, or sulphuretted hydrogen were concerned, I have used muriatic acid saturated with sulphuretted hydrogen over mercury.

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* This circumstance has been pointed out by MM. Gay Lussac and Thenard, in a paper printed in the Journal de Physique for December, in which these gentlemen endeavour to shew that, whether potassium has been acted upon by large or small quantities of sulphur, and under all circumstances, it evolves a quantity of gas exactly equal to that which it produces by the action of water. I have been able to gain no results so precise on this subject. I have in another place (the same journal in which their memoir has appeared) offered some observations on their inquiries. [Journal de Physique, Mai. 1810.—I have not thought it advisable to insert the answer, alluded to in the above note; as the principal facts, the results of experiments adduced in reply to MM. Gay Lussac and Thenard, are brought forward in this paper.]

I have employed sulphur distilled from iron pyrites in vacuo, which did not in the slightest degree affect litmus paper, and I have combined it with potassium in retorts of green glass or plate glass, lined with sulphur and filled with very pure nitrogen or hydrogen. In making potassium act upon sulphuretted hydrogen, I have employed the gas only in the quantities of from 1 to 3 cubical inches, and have made the combination in narrow curved tubes of green glass over dry mercury. With all these precautions, and after having made a great number of experiments, I am not able to gain perfectly uniform results. Yet there is sufficient correspondence between them to enable me to form conclusions, which I may venture to say cannot be far from the truth.

When 1 grain of potassium, which would give by the action of water about 1 cubical inch and $\frac{1}{10}$ of hydrogen is made to act upon about half a grain of sulphur, some sulphur sublimes during the combination, which always takes place with heat and light, and from $\frac{1}{14}$ to $\frac{1}{10}$ of a cubical inch of sulphuretted hydrogen is evolved. The compound acted on by muriatic acid, saturated with sulphuretted hydrogen, affords from $\frac{9}{10}$ to $\frac{11}{10}$ of a cubical inch of pure sulphuretted hydrogen.

When more sulphur is used so as to be from twice to ten times the weight of the potassium, the quantity of sulphuretted hydrogen evolved by the action of the acid, is from $\frac{7}{10}$ to $\frac{9}{10}$: but if heat be applied to the combination, so as to drive off the superfluous sulphur, the quantity of gas collected is very little inferior to that produced from the combination in which a small proportion of sulphur is used; and I am inclined to believe, from the phenomena presented in a great number of experiments, that sulphur and potassium, when heated together under common circumstances, combine only in one proportion, in which the metal is to the sulphur, nearly as 3 to 1 in weight; and in which the quantities are such that the compound burns into neutral sulphate of potash.

When a grain of potassium is made to act upon about 1·1 cubical inch of sulphuretted hydrogen, all the hydrogen is set free, and a sulphuret of potassium containing one-fourth of sulphur is formed, exactly the same as that produced by the immediate combination of sulphur and the metal.

When sulphuretted hydrogen is employed in larger quantities, there is an absorption of this gas, and a volume is taken up about equal to the quantity of hydrogen disengaged, and a compound of sulphuretted hydrogen and sulphuret of potash is formed, which gives sulphuretted hydrogen by the action of an acid, nearly double in quantity to that given by the sulphuret of potassium.

From a number of experiments, I am inclined to believe that potassium and phosphorus, in whatever quantities they are heated together, combine only in one proportion, a grain of potassium requiring about $\frac{3}{8}$ of a grain of phosphorus to form a phosphuret; which when acted upon by muriatic acid, produces from $\frac{8}{10}$ to $\frac{10}{10}$ of a cubical inch of phosphuretted hydrogen.

Half a grain of potassium decomposes nearly three cubical inches of phosphuretted hydrogen, and sets free rather more than four cubical inches of hydrogen; and the phosphuret formed seems to be of the same kind as that produced by direct combination of the metal with phosphorus.

If, according to Mr. Dalton's idea of proportion, the quantity in which sulphur enters into its combinations, were to be deduced from its union with potassium, in which it seems to form about $\frac{1}{4}$ the weight of the compound, the number representing it would be 13·5. I have lately weighed sulphuretted hydrogen and sulphurous acid gas with great care. The specific gravity of the first at mean temperature and pressure, from my experiments, is 10645,* which differs very little from the estimation of Mr. Kirwan: that of sulphurous acid gas, I find, is 20967. Sulphuretted hydrogen, as I have shown, contains an equal volume of hydrogen; and on this datum the number representing sulphur is 13·4. I have never been able to burn sulphur in oxygen without forming sulphuric acid in small quantities; but in several experiments I have obtained from 92 to 98 parts of sulphurous acid, from 100 of oxygen in volume; from which I am inclined to believe, that sulphurous acid consists of sulphur dissolved in an equal volume of oxygen; which would give the number as 13·7† nearly, considering the acid gas as containing 1 proportion of sulphur and 2 of oxygen; and these estimations do not differ from each other materially.

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* [Considering atmospheric air as 10,000.]

† This estimation, from the composition of sulphuretted hydrogen, must be considered as most accurate, and that from the formation of the sulphuret of potassium as least accurate: for it was only by combining sulphur and potassium in small proportions, and ascertaining in what cases uncombined sulphur could be distilled from the compound, that I gained my conclusions concerning the composition of the sulphuret of potassium.

In the last Bakerian Lecture I have estimated the specific gravity of sulphuretted hydrogen at 35 grains the 100 cubical inches, which was not far from the mean between the estimations of Mr. Kirwan and M. Thenard. According to the last experiment, sulphuretted hydrogen is composed of 1 proportion of hydrogen, represented by 1, and 1 of sulphur, represented by 13·4. [The author afterwards found the weight of 100 cubic inches of this gas to be 36·5 grains.]

I have made several experiments on the combustion of phosphorus in oxygen gas. From the most accurate I am inclined to conclude, that 25 of phosphorus absorb in combustion about 34 of oxygen in weight; and considering phosphoric acid as composed of 3 proportions of oxygen and 1 of phosphorus, the number representing phosphorus will be about 16·5,* which is not very remote from the number that may be deduced from the composition of phosphuret of potassium.

The numbers which represent the proportions in which sulphur and phosphorus unite with other bodies, are such as do not include the existence of combined portions of oxygen and hydrogen in their constitution; but it may be questioned whether the opinion which I formed, that the inflammable gas disengaged from them by electricity, is necessary to the peculiar form in which these bodies exist, is not erroneous. Phosphorus, as I have stated in the last Bakerian Lecture, is capable of forming a solid hydruret: and a part of the sulphur distilled from iron pyrites is usually of a soft consistence, and emits the smell of sulphuretted hydrogen, and probably contains that body. It is not unlikely that in all cases, phosphorus and sulphur contain small quantities of the hydrurets of phosphorus and sulphur; and the production of a minute portion of sulphuric acid in the slow combustion of sulphur, is probably connected with the production of water. Though the pure oxides of sulphur and phosphorus have never been obtained; yet from the doctrine of definite proportions, these bodies ought, under certain circumstances, to be formed. And I am inclined to believe that they sometimes exist in minute quantities in common phosphorus and sulphur, and with hydrogen, give to them their variable properties.

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* [The author from his later researches on the combinations of phosphorus and oxygen in 1818, he deduced that the number representing the former is 22·5.]

The colours of different specimens of phosphorus, as well as of sulphur, differ considerably; the red colour of phosphorus, as it is commonly prepared, is probably owing to a slight mixture of oxide. Common roll-sulphur is of a very pale yellow, the Sicilian sulphur of an orange colour, and the sulphur distilled from iron pyrites in vacuo, which arose in the last period of the process, of a pale yellowish-green colour. All the late experiments that I have made, as well as my former researches, induce me to suspect a notable proportion of oxygen in Sicilian sulphur,* which is probably owing to the presence of oxide of sulphur, which may give rise to sulphurous acid in distillation, or to sulphuric acid itself.

Conceiving that if definite proportions of oxygen and hydrogen existed in sulphur and phosphorus, they ought to be manifested in the agency of oxymuriatic acid gas on these bodies, I made some experiments on the results of these operations. In the first trial on the combination of sulphur with oxymuriatic acid gas, I employed 5 grains of roll-sulphur, and admitted the gas into the exhausted retort from a vessel in which it had been in contact with warm water: in this case, more than half a cubical inch of oxygen gas, and nearly two cubical inches of muriatic acid gas were produced. Suspecting in this instance that aqueous vapour had been decomposed, I employed cold water in the next experiment, and dried the gas by muriate of lime; in this case, though Sicilian sulphur was used, no oxygen gas was evolved; and not half a cubical inch of muriatic acid; the quantity was the same as in the last experiment; and it was found that between 16 and 17 cubical inches of oxymuriatic acid gas had disappeared: the whole of the sulphur was sublimed in the gas, and the liquor formed was of a tawny-orange colour.

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* [Considering the manner in which sulphur is procured in Sicily, it might be expected to contain oxygen; when taken from the mine, the limestone rock containing it, broken into small fragments, is subjected to heat in a kind of kiln; whilst a small portion of the sulphur is burnt, and ascends into the atmosphere in the form of sulphurous acid gas, the greater part of it melts, sinks, and flows out through an opening designed to give issue. This process I witnessed at the extensive sulphur mines in the neighbourhood of Gujenti; and I believe it is generally in use throughout the sulphur districts.]

No oxygen was expelled during the combustion of phosphorus in oxymuriatic acid gas, nor could I ascertain that any muriatic acid had been formed; 3 grains of phosphorus were entirely converted into sublimate, by the absorption of about 23 cubical inches and a half of the gas. It would seem from these quantities, that the sulphuretted liquor formed by subliming sulphur in oxymuriatic acid gas, consists of 1 proportion of sulphur, represented by 13·5, and 1 of oxymuriatic gas represented by 32·9, and that the phosphoric sublimate must be composed of 3 portions of oxymuriatic gas, represented by 98·7, and 1 of phosphorus represented by 16·5.

VIII.

THE BAKERIAN LECTURE. ON SOME OF THE COMBINATIONS OF OXYMURIATIC GAS AND OXYGEN, AND ON THE CHEMICAL RELATIONS OF THESE PRINCIPLES, TO INFLAMMABLE BODIES.

I. *Introduction.*

IN the last communication which I had the honour of presenting to the Royal Society, I stated a number of facts, which inclined me to believe, that the body, improperly called in the modern nomenclature of chemistry, *oxymuriatic acid gas*, has not as yet been decompounded; but that it is a peculiar substance, elementary as far as our knowledge extends, and analogous in many of its properties to oxygen gas.

My objects in the present Lecture, are to detail a number of experiments which I have made for the purpose of illustrating more fully the nature, properties, and combinations of this substance, and its attractions for inflammable bodies, as compared with those of oxygen; and likewise to present some general views and conclusions concerning the chemical powers of different species of matter, and the proportions in which they enter into union.

I have been almost constantly employed, since the last session of the Society, upon these researches, yet this time has not been sufficient to enable me to approach to any thing complete in the investigation. But on subjects, important both in their connection with the higher departments of chemical philosophy, and with the economical applications of chemistry, I trust that even these imperfect labours will not be wholly unacceptable.

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* [From Phil. Trans. for 1811. Read before the Royal Society, Nov. 15, 1810.]

II. *On the Combinations of Oxymuriatic Gas and Oxygen, with the Metals from the fixed Alkalies.*

The intensity of the attraction of potassium for oxymuriatic gas, is shewn by its spontaneous inflammation in that substance, and by the vividness of the combustion. I satisfied myself, by various minute experiments, that no water is separated in this operation, and that the proportions of the compound are such that one grain of potassium absorbs about 1·1 cubical inch of oxymuriatic gas at the mean temperature and pressure, and that they form a neutral compound, which undergoes no change by fusion. I used, in the experiments from which these conclusions are drawn, a tray of platina for receiving the potassium: the metal was heated in an exhausted vessel, to decompose any water absorbed by the crust of potash, which forms upon the potassium during its exposure to the atmosphere, and the gas was freed from vapour by muriate of lime. Large masses of potassium cannot be made to inflame, without heat in oxymuriatic gas. In all experiments in which I fused the potassium upon glass, the retorts broke in pieces in consequence of the violence of the combustion, and even in two instances when I used the tray of platina. If oxymuriatic gas be used, not freed from vapour, or if the potassium has been previously exposed to the air, a little moisture always separates during the process of combustion. When pure potassium, and pure oxymuriatic gas are used, the result, as I have stated, is a mere binary compound, the same as muriate of potash, that has undergone ignition.

The combustion of potassium and sodium in oxygen gas is much less vivid than in oxymuriatic gas. From this phenomenon, and from some others, I was inclined to believe that the attraction of these metals for oxygen is feebler than their attraction for oxymuriatic gas. I made several experiments, which proved that this is the fact; but before I enter upon a detail of them, it will be necessary to discuss more fully than I have yet attempted, the nature of the combinations of potassium and sodium with oxygen, and of potash and soda with water.

I have stated in the last Bakerian Lecture, that potassium and sodium, when burnt in oxygen gas, produce potash and soda in a state of extreme dryness, and very difficult of fusion. In the experiments from which these conclusions are drawn, as I mentioned, I used trays of platina, and finding that this metal was oxidated in the operation, I heated the retort strongly, to expel any oxygen the platina might have absorbed, and except in cases when this precaution was taken, I found the absorption of oxygen much greater than could be accounted for by the production of the alkalies. In all cases in which I burnt potassium or sodium in common air, applying only a gentle heat, I found that the first products were substances extremely fusible, and of a reddish brown colour, which copiously effervesced in water, and which became dry alkali, by being strongly heated upon platina in the air; phenomena, which, at an early period of the inquiry, induced me to suppose that they were protoxides of potassium and sodium. Finding, in subsequent experiments, however, that they deflagrated with iron filings, and rapidly oxidated platina and silver, I suspended my opinion on the subject, intending to investigate their nature more fully.

Since that time, these oxides, as I find by a notice in the Moniteur for July 5th, 1810, have occupied the attention of MM. Gay Lussac and Thenard, and these able chemists have discovered that they are peroxides of potassium and sodium, the one containing, according to them, three times as much oxygen as potash, and the other 1·5 times as much as soda.

I have been able to confirm in a general way these interesting results, though I have not found any means of ascertaining accurately, the quantity of oxygen contained in these new oxides. When they are formed upon metallic substances, there is always a considerable oxidation of the metal, even though platina be employed. I have used a platina tray lined with muriate of potash, that had been fused; but in this case, though I am inclined to believe that some alkali was formed at the same time with the peroxides, yet I obtained an absorption of 2·6 cubical inches, in a case when 2 grains of potassium were employed, and of 1·63 cubical inches, in a case when a grain of sodium was used, but in this last instance, the edge of the platina tray had been acted upon by the metal, and was oxidated.* The mercury in the barometer in these experiments stood at 30·12 inches, and that in the thermometer at 62° Fahrenheit.

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* MM. Gay Lussac and Thenard have stated in the paper above referred to, that common potash and barytes absorb oxygen when heated. It would seem that the action of the fixed alkalies, and of barytes on platina, depends on the production of the peroxides. I have little doubt but that these ingenious gentlemen will have anticipated this observation, in the detailed account of their experiment.

When these peroxides were formed upon muriate of potash, the colour of that from potassium was of a bright orange; that from sodium of a darker orange tint. They gave off oxygen, as MM. Gay Lussac and Thenard state, by the action of water or acids. They were converted into alkali, as the French chemists have stated, by being heated with any metallic or inflammable matter. They thickened fixed oils, forming a compound that did not redden paper tinged with turmeric, without the addition of water.

When potassium is brought in contact with fused nitre, in tubes of pure glass, there is a slight scintillation only, and the nitre becomes of a red brown colour. In this operation, nitrogen is produced, and the oxide of potassium formed. I thought that by ascertaining the quantity of nitrogen evolved by the action of a given weight of potassium, and comparing this with the quantity of oxygen disengaged from the oxide by water, I might be able to determine its composition accurately.

A grain of potassium acting in this way, I found produced only $\frac{16}{100}$ of nitrogen; and the red oxide by its action upon water, produced less than half a cubical inch of oxygen, so that it is probable that potash as well as its peroxide is formed in the operation.

Sodium, when brought in contact with fused nitre, produced a violent deflagration. In two experiments in which I used a grain of the metal, the tube broke with the violence of the explosion. I succeeded in obtaining the solid results of the deflagration of half a grain of sodium, but it appeared that no peroxide had formed, for the mass gave no oxygen by the action of water.

When potassium is burnt in a retort of pure glass, the result is partly potash and partly peroxide, and by a long continued red heat the peroxide is entirely decomposed.

A grain of potassium was gently heated in a small green glass retort containing oxygen; it burnt slowly, and with a feeble flame; a quantity of oxygen was absorbed equal to $\frac{90}{100}$ of a cubical inch; by heating the retort to dull redness, oxygen was expelled equal to $\frac{38}{100}$ of a cubical inch; the mercury in the thermometer in this experiment stood at 63° Fahrenheit, and that in the barometer at 30·1 inches.

In experiments on the electrical decomposition of potash and soda, when the Voltaic battery employed contains from 500 to 1000 series in full action; the metals burn at the moment of their production, and form the peroxides; and it is probable, from the observations of M. Ritter, that these bodies may be produced likewise in Voltaic operations on potash, at the positive surface.

In my early experiments on potassium and sodium, I regarded the fusible substances appearing at the negative surface, in the Voltaic circuit, as well as those produced by the exposure of the metals to heat and air, as protoxides, and as similar to the results obtained by heating the metals in contact with small quantities of alkali.

I have repeated these last operations, in which I conceived that protoxides were formed.

Potassium and sodium, when heated in glass tubes in contact with about half their weight of potash and soda, that have been ignited, become first of a bright azure, then produce a considerable quantity of hydrogen, and at last form a gray coherent mass, not fusible at a dull red heat, and which gives hydrogen by the action of water.

Whether these are true protoxides, or merely mixtures of the alkaline metals with the alkalies, or with the alkalies and reduced silex from the glass, I shall not at present attempt to decide.

Potassium, I find, heated in a similar manner with fused potash, in a tube of platina, gives, after having been ignited, a dark mass that effervesces with water; but even in this case, it may be said that the alloy of platina and potassium interferes, and that the substance is not a protoxide, but merely dry alkali mixed with this alloy.

As the pure alkalies were unknown, till the discovery of potassium and sodium,* and as their properties have never been described, it will perhaps be proper in this place to notice them briefly.

When potassium and sodium are burnt in oxygen gas upon platina, and heated to redness to decompose the peroxide of potassium, the alkalies are of a grayish green colour. They are harder than common potash or soda, and, as well as I could determine by an imperfect trial, of greater specific gravity. They require a strong red heat for their perfect fluidity, and evaporate slowly, by a still further increase of temperature. When small quantities of water are added to them, they heat violently, become white, and are converted into hydrats, and then are easily fusible and volatile.

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* Stahl approached nearly to the discovery of the pure alkalies. He cemented solid caustic potash with iron filings in a long continued heat, and states, that in this way an alkali "valde causticum" is produced. *Specim. Bech.* part ii. page 255. He procured caustic alkali also, by decomposing nitre by the metals. Id. p. 253.

I find that when nitre is decomposed in a crucible of platina, by a strong red heat, a yellow substance remains, which consists of potash and oxide of platina, apparently in chemical combination. The undecompounded potash which comes over in the process for procuring potassium by the gun-barrel, is of an olive colour, and affords oxide of iron during its solution in water. Pure potash will probably be found to have an affinity for many metallic oxides.

When potassium or sodium is burnt on glass, freed from metallic oxides, and strongly heated, or when potash or soda is formed from the metals by the action of a minute quantity of water, their colour approaches to white; but in other sensible properties, they resemble the alkalies formed upon metallic substances; and are distinguished in a marked manner by their difficult fusibility from the potash and soda prepared by alcohol.

M. D'Arcet, and more distinctly M. Berthollet, have concluded that the loss of weight of common fused potash and soda, during their combination with acids, depends upon the expulsion of water, which M. Berthollet has rated at 13·9 per cent. for potash, and M. D'Arcet, at 27 or 28 for potash, and 28 or 29 for soda.*

I have stated, in the last Bakerian Lecture, that my own results led me to conclude, that fused potash contained about 16 or 17 parts in the 100 of water, taking the potash formed, by adding oxygen to potassium as a standard.

The experiment from which I drew my conclusions, was made on the action of silex and potash fused together, and I regarded the loss of weight, as the indication of the quantity of moisture.

I am acquainted with no experiment on record, in which water has been actually collected from the ignited fixed alkalies, and this appeared necessary for the complete elucidation of the subject.

I heated together in a green glass retort, 40 grains of potash (that had been ignited for several minutes), and 100 grains of boracic acid, which had been heated to whiteness for nearly an hour. The retort was carefully weighed, and connected with a small receiver, which was likewise weighed; the bulb of the retort was then gradually heated till it became of a cherry red; there was a violent effervescence in the retort, a fluid condensed in the neck, and passed into the receiver. When the process was completed, the whole of the retort was strongly heated; it was found to have lost 6½ grains, and the receiver had gained 5·8 grains. The fluid that it contained was water, holding in solution a minute quantity of boracic acid, and when evaporated, it did not leave an appreciable quantity of residuum.

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* Annales de Chimie, tom. 68, page 190.

A similar experiment made upon soda, heated to redness, but in which the water collected was not weighed, indicated 22·9 of water in 100 parts of soda.

It may be asked, whether part of the water evolved in these processes might not have been produced from the boracic acid, or formed in consequence of its agency; but the following experiments shew that this cannot be the case in any sensible degree.

I heated 8 grains of potassium, with about 50 grains of boracic acid, to redness in a tube of platina, connected with a glass tube, kept very cool; but I found that no moisture whatever was separated in the process. I mixed a few grains of potassium with red oxide of mercury, and ignited the mixture in contact with boracic acid, but no elastic product, except mercury, was evolved.

I made some potash by the combustion of potassium in a glass tube, and ignition of the peroxide; I added to it dry boracic acid, and heated the mixture to redness. Sub-borate of potash was formed, and there was not the slightest indication of the presence of moisture.*

It is evident from this chain of facts, that common potash and soda are hydrats, and the bodies formed by

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* These processes must not however be considered as shewing that boracic acid that has been heated to whiteness is entirely free from water; they merely prove that such an acid gives off no water by combination with pure potash at a *red heat*. I have found that boracic acid in perfect fusion, and that has been long exposed to the blast of a forge, and that has long ceased to effervesce, gives globules of hydrogen; when dry iron filings are made to act upon it. I added to 54 grains of boracic acid in complete fusion, in a crucible of platina, 75 grains of flint glass that had been previously heated to whiteness, and immediately reduced into powder in a hot iron mortar; by raising the heat so as to produce combination, a copious effervescence was produced; and after intense ignition for half an hour, the mixture was found to have lost three grains and a quarter.

The combinations of boracic acid with potash and soda, that have been heated to redness, I find lose weight when their temperature is raised to a much higher degree. Thus, in an experiment made in the laboratory of my friend John George Children, Esq, and in which Mr. Children was so kind as to co-operate, 71 grains of hydrat of potash, mixed with 96 of boracic acid that had been heated as strongly as possible in a blast furnace, lost by fusion together in a red heat 11 grains, but on raising the temperature to whiteness the loss increased to above 13 grains. 55·5 grains of hydrat of soda, mixed with 80 of boracic acid, examined at intervals in a process of this kind, continued to lose weight for half an hour, during which time they were frequently heated to whiteness; at the end of this period the whole loss was 14 grains, of which at least one grain and a half may be referred to the acid. 95 grains of soda, ignited to whiteness in a platina crucible, with 140 of dry flint glass, lost 22·2 grains; 80 grains of boracic glass were added to this mixture; a fresh effervescence took place, and after intense ignition for a few minutes, there was an additional loss of weight of four grains and a half. The energy with which water adheres to certain bodies in other cases, is shewn by the experiments of M. Berthollet, *Mem. d'Arcueil*, tom. ii. page 47. Indeed it is impossible to say that a neutral compound, or a fixed acid is ever entirely free from water; it is only the first proportions that are easily separated. If the proportions of water in common potash and soda were to be judged of from their loss of weight, in combining with boracic acid, it would apthe combustion of the alkaline metals, are, as I have always stated, pure metallic oxides, (as far as our knowledge extends) free from water.*

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pear to be from 19 to 20 per cent. in the first, and from 23 to 25 in the second.

* After the experiments detailed in my two last papers, it may perhaps appear unnecessary, at least to those enlightened British chemical philosophers who have closely followed the progress of science, to offer any new evidences to prove that potassium and sodium are not hydrurets of potash and soda, particularly as MM. Gay Lussac and Thenard, the ingenious advocates of this notion have acknowledged, in the Moniteur to which I have before referred, that it is not tenable; but on a subject so intimately connected with the most refined departments of chemical philosophy, and with so many new objects of research, additional facts cannot be wholly devoid of use and application.

Mr. Dalton, in the second volume of the work which he entitles "*A New System of Chemical Philosophy*," of which he has had the goodness to send me a copy, has, I find in his first pages, adopted the idea that potash and soda are metallic oxides; but in the latter pages has considered them as simple bodies, and the metals formed from them as compounds of potash and soda with hydrogen. He has given no facts in favour of this change in his opinion: his principal argument is founded upon the process in which I first obtained potassium. Common potash is a hydrat: when oxygen is procured from this by Voltaic electricity at one surface, and potassium at the other surface; Mr. Dalton conceiving that this oxygen arises from the water, states that the hydrogen of the water must combine with the potash to form potassium. It is evident, that adopting such a plan of reasoning, lead or copper might be proved to be hydrurets of their oxides; for when these metals are revived from their aqueous acid solutions, oxygen is produced at the positive surface, and no hydrogen at the negative surface.

In my first experiments for producing potassium and sodium, I used a weak power, and in these instances procuring the metals in very small quantities only, I perceived no effervescence. When from five hundred to one thousand plates are used for producing potassium, there is a violent effervescence, and a production of hydrogen and sometimes of potassuretted hydrogen, connected with the formation of the metal.

Potassium, brought in contact with red-hot hydrat of potash, disengages abundance of hydrogen, and the whole is converted into difficultly fusible potash.

327 grains of hydrat of potash that had been ignited, were made to

I shall now resume the detail of the experiments that I have made, on the relative attractions of oxymuriatic gas and oxygen, for the metals of the fixed alkalies. I burnt a grain of potassium in oxygen gas, in a retort of green glass, furnished with a stop-cock, and heated the oxide formed, to redness, to convert it into potash: half a cubical inch of oxygen was absorbed. The retort

act in a gun-barrel on 745 grains of iron turnings heated to whiteness. Some hydrogen was lost, and some hydrat of potash remained undecompounded, yet 225 cubical inches of inflammable gas were collected, and 50 grains of potassium, and a large quantity of an alloy of potassium and iron formed, so that it is scarcely possible to doubt that all the hydrogen produced from the decomposed hydrat of potash was liberated.

Mr. Dalton conceives that there is an analogy between potassium and sodium, and the compounds of hydrogen with sulphur, phosphorus, and arsenic; but I am at a loss to trace any similarity between sulphuretted hydrogen, which is a gaseous body, soluble in water, and having acid properties, and a highly inflammable solid metal which produces alkali by combustion. Potassium might as well be compared to carbonic acid. Mr. Dalton considers the volatility of potassium and sodium as favouring the idea of their containing hydrogen; but they are less volatile than antimony, arsenic, and tellurium, and much less volatile than mercury. He mentions their low specific gravity as a circumstance favourable to this idea. I have on a former occasion examined this argument, first brought forward by M. Ritter: but it may not be amiss to add, that if potassium is a compound of hydrogen and potash, hydrat of potash must contain an equal quantity of hydrogen, with the addition of a light gaseous element, oxygen, which might be expected to diminish rather than to increase the specific gravity of the compound. Mr. Dalton states, p. 488, that potassium forms dry hydrat of potash, by decomposing nitrous gas and nitrous oxide; this is not the case: and he does not refer to experiment. I find by some very careful trials, that potassium attracts the oxygen and some of the nitrogen from these bodies, and forms a fusible compound which may be decomposed, giving off nitrogen and its excess of oxygen, by a red heat, and which becomes *potash*, and not dry hydrat of potash.

MM. Gay Lussac and Thenard have convinced themselves that potassium and sodium are not hydrurets of potash and soda, by a method similar to that which I adopted and published some months before, namely, by producing neutral salts from them.

was exhausted, and very pure oxymuriatic gas admitted. The colour of the potash instantly became white, and by a gentle heat, the whole was converted into muriate of potash: a cubical inch and $\frac{1}{8}$ of oxymuriatic gas were absorbed, and exactly half a cubical inch of oxygen generated. The barometer during this operation was at 30·3, the thermometer at 62° Fahrenheit.

I made several experiments of the same kind, but this is the only one on which I can place entire dependence. When I attempted to use larger quantities of potassium, the retort usually broke during the cooling of the glass, and it was not possible to gain any accurate results in employing metallic trays. The potassium was spread into a thin plate, and of course was much oxidated before its admission into the retort, which rendered the absorption of oxygen a little less than it ought to have been. In the process it was heated in vacuo before the combustion, to decompose the water in the crust of potash; for in cases when this precaution was not taken, I found that hydrat of potash sublimed, and lined the upper part of the retort, and from this the oxymuriatic gas separated water as well as oxygen.

The phenomenon of the separation of water from hydrat of potash by oxymuriatic gas, was happily exemplified in an experiment in which I introduced oxymuriatic gas to the peroxide of potassium, formed in a large retort, and in which the potassium had been covered with a considerable crust of hydrat of potash. The upper part of the retort and its neck contained a white sublimate of hydrat, which had risen in combustion, and which was perfectly opaque. As soon as the gas was admitted, it instantly became transparent from the evolution of water; and on heating the glass in contact with the sublimate, its opacity was restored, and water driven off.

In various cases in which I heated dry potash, or mixtures of potash and the peroxide, in oxymuriatic gas, there was no separation of moisture, except when the gas contained aqueous vapour; and the oxygen evolved in the process, when the heat was strongly raised, exactly corresponded to that absorbed by the potassium.

When muriatic acid gas was introduced to potash formed from the combustion of potassium, water was instantly formed, and oxymuriate of potassium.* I have made no accurate experiment on the proportions of muriatic acid gas decomposed by potash, but I made a very minute investigation, of the nature of the mutual decomposition of this substance, and hydrat of potash.

Ten grains of hydrat of potash were heated to redness in a tray of platina, which was carefully weighed; it was introduced into a retort which was exhausted of air, and the retort was filled with muriatic acid gas. The hydrat of potash was heated by a spirit lamp; water instantly separated in great abundance, and muriate of potash formed. A strong heat was applied till the process was completed, when the tray was taken out and weighed; it had gained $2\frac{3}{16}$ grains. A minute quantity of liquid muriatic acid was added to the muriate, to ensure a complete neutralization, and the tray heated to redness: there was no additional increase of weight.

In the few experiments which I have made on the action of sodium and soda on oxymuriatic gas, the phenomena appeared precisely analogous; but sodium, as might have been expected, absorbed nearly twice as much oxymuriatic gas as potassium.

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* i. e. Muriate of potash. [Chloride of potassium.]

When common salt that has been ignited, is heated with potassium, there is an immediate decomposition, and by giving the mixture a red heat, pure sodium is obtained;* and this process affords an easy mode, and the one I have always lately adopted for procuring that metal. No hydrogen is disengaged in this operation, and two parts of potassium, I find, produces rather more than one of sodium.

From the series of proportions that I have communicated in my last paper, it is evident that 1 grain of potassium ought to absorb 1·08 cubical inches of oxymuriatic acid; and that the potash formed from one grain of potassium ought to decompose about 2·16 cubical inches of muriatic acid gas; and these estimations agree very nearly with the result of experiments.

The estimation of the composition of soda, as deduced from the experiments in the last Bakerian Lecture, is 25·4 of oxygen to 74·6 of metal, and this would give the number representing the proportion in which sodium combines with bodies 22;† from which it is evident, that a grain of sodium ought to absorb nearly 2 cubical inches of oxymuriatic gas, and that the same quantity converted into soda, would decompose nearly four cu-

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* [By a similar method, acting on the chlorides by potassium, the metallic bases of magnesia, alumine, glucine, and yttria have been obtained and examined in a state of purity.]

† Or if soda be considered as deutoxide, which seems probable from the experiments detailed page 4, 44; and on this supposition, the salts of soda must be conceived to contain double proportions of acid. On either datum the proportion of oxygen in water must be taken as 7·5, and that of hydrogen as 1, though other numbers might be found as divisors or multiples of those which would equally harmonise with the general doctrine of definite proportions. In my last communication to the Society, I have quoted Mr. Dalton as the original Author of the hypothesis, that water consists of 1 particle of oxygen, and 1 of hydrogen; but I have since found that this opinion is advanced, in a work published in 1789—*A Comparative View of the Phlogistic and Antiphlogistic Theories*, by William Higgins. In this elaborate and ingenious performance, Mr. Higgins has developed many happy sketches

of the manner in which (on the corpuscular hypothesis) the particles or molecules of bodies may be conceived to combine; and some of his views, though formed at this early period of investigation, appear to me to be more defensible, assuming his data, than any which have been since advanced; for instance, he considered nitrous gas as composed of two particles of oxygen, and one of nitrogen. Mr. Higgins had likewise drawn the just conclusion respecting the constitution of sulphuretted hydrogen, from its electrical decomposition. As hydrogen is the substance which combines with other bodies in the smallest quantity, it is perhaps the most fitted to be represented by unity; and on this idea, the proportions in ammonia will be 3 of hydrogen to 1 of nitrogen, and the number representing the smallest proportion in which nitrogen is known to combine will be 13·4. Mr. Dalton, *New System of Chemical Philosophy*, pages 323 and 436, has adopted 4·7 or 5·1, as the number representing the weight of the *atom* of nitrogen; and has quoted my experiments, *Researches, Chemical and Philosophical*, as authorising these numbers; but all the inquiries on nitric acid, nitrous gas, nitrous oxide, and on the decomposition of nitrate of ammonia stated in that work, conform much more nearly to the number 13·4.

According to Mr. Dalton, nitrate of ammonia contains one proportion of acid and one of alkali, and nitrate of potash two proportions of acid and one of alkali; but it is easy to see that the reverse must be the case. Nitrate of ammonia is known to be an acid salt; and nitrate of potash a neutral salt; which harmonizes with the views above stated. Mr. Dalton estimates the quantity of water in nitric acid of specific gravity 1·54, at 27·5 per cent.; and this, according to him, is a stronger acid than he obtained by decomposing fused nitre by sulphuric acid, which contained only 19 per cent. of water, and one quantity of sulphuric acid, according to him, will produce from nitre more than an equal weight of nitric acid, and he supposes no water in nitre; so that his conclusion as to the quantity of water in liquid nitric acid on his own data must be incorrect. I find water in fused nitre, by decomposing it by boracic acid.

I shall enter no further at present into an examination of the opinions, results, and conclusions of my learned friend; I am however obliged to dissent from most of them, and to protest against the interpretations that he has been pleased to make of my experiments; and I trust to his judgment and candour for a correction of his views.

bical inches of muriatic gas. Muriate of soda ought on this idea to contain one proportion of sodium, 22, and one of oxymuriatic gas 32·9; and this estimation is

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It is impossible not to admire the ingenuity and talent with which Mr. Dalton has arranged, combined, weighed, measured, and figured his atoms; but it is not, I conceive, on any speculations upon the ultimate particles of matter, that the true theory of definite proportions must ultimately rest. It has a surer basis in the mutual decomposition of the neutral salts, observed by Richter and Guyton de Morveau, in the mutual decompositions of the compounds of hydrogen and nitrogen, of nitrogen and oxygen, of water and the oxymuriatic compounds; in the multiples of oxygen in the nitrous compounds; and those of acids in salts, observed by Drs. Wollaston and Thomson; and above all, in the decompositions by the Voltaic apparatus, where oxygen and hydrogen, oxygen and inflammable bodies, acids and alkalies, &c., must separate in uniform ratios. [The author, in the discourse which he delivered in awarding a Royal Medal to Dr. Dalton in 1826, did him ample justice as the discoverer of the practical and valuable part of the atomic theory in application to chemistry, namely, the doctrine of multiples in combination. He at first over-rated the merit of Mr. Higgins, in relation to his views concerning the atomic doctrine, and gave him credit for originality in speculation, which was rather the due of Dr. Bryan Higgins, to whom also he did justice in the same discourse.

[The author, it has been said, in the above note offered a kind of factious opposition to the atomic views of Dr. Dalton. Such opposition was very foreign from his nature. It must be admitted, that he expressed his objection to Dr. Dalton's hypothetical views respecting atoms, too lightly; I will not say too strongly, because the opinion which he had of them then, I believe he always retained; in corroboration, I shall give some extracts from one of his unfinished Dialogues, written when he was in Italy, entitled, " On the Powers which act upon Matter, and produce Chemical changes." After stating some of the leading facts on which definite chemistry rests, he remarks:—]

The fact of the combination of bodies in definite proportions, has led to the revival of that ancient doctrine of the Greek school, attributed to Democritus, that the real elements of bodies are certain indestructible particles, or atoms, from the arrangements of which, in various forms and combinations, all the varieties of matter result. This doctrine which, in its application to chemistry, has been called the Atomic doctrine, or theory, has been embraced by several modern chemists; but the development of it is owing to Mr. Dalton, who seems to have been very near that which may be gained from Dr. Marcet's analysis of this substance. Hydrat of potash ought to consist of one proportion of potash, represented by 48,

the first person to generalize the facts of chemistry relating to definite proportions, and to draw this inference from them.

Pol. If I am not mistaken, Newton, in his optics, has given a view of this kind, and most of the great mechanical philosophers seem to have entertained some similar notions.

Philochemicus. You are in the right; but until lately the idea was more a metaphysical generalization than a probable inference from facts; and all the arguments supporting it are derived from the modern discoveries in chemistry.

[In continuation, having given instances of simple and compound chemical attraction, in connection with the doctrine of definite proportions, he proceeds :]

Of course, in all instances of complex, as of simple attraction, the results may be known by calculation; for the different substances must always separate and unite in the relative proportions in which they exist in the compounds. Indeed, the demonstrations of this by Richter, may be said to have laid the foundation of definite proportions; and however Dalton may be considered as the reviver of the atomic system,—the author of the application of the hypothesis to this doctrine, and illustrator of it by the example of multiples, where bodies combine in more than one proportion; still a prior claim belongs to Richter, who, by determining the law of the relations of the proportions, in all the compounds of the same body, must be considered as the founder of definite chemistry.

Pol. In developing the atomic system, you have mentioned only the names of Dalton and of Richter: now I remember some years ago hearing a gentleman lay claim to this doctrine, Mr. William Higgins, Professor of Chemistry to the Dublin Society; and he referred to a book, published at least ten years before Mr. Dalton's papers, and I believe, even before Richter's papers, called " A Comparative View," &c.

Philochemicus. You are perfectly right; the claim has been urged; and the book certainly entertains views which support it: but Mr. W. Higgins appears to have had only some loose ideas of particles combining with particles, without any profound views of the quantity being unalterable; and there is good reason for thinking that these ideas, as he expresses them, were gained from another source, Dr. Bryan Higgins, who many years before supported the notion, that chemical substances were formed of molecules, either simple or compound, surrounded by an and one of water, represented by 8·5. This gives its composition as 15·1 of water, and 84·9 of potash. Hydrat of soda ought, according to theory, to contain 1 proportion of soda 29·5, and 1 of water 8·5, which will give in 100 parts 22·4 of water; and the experiments that I have detailed, conform as well as can be expected with these conclusions.

The proportions of potash and soda indicated, in different neutral combinations, by these estimations, will be found to agree very nearly with those derived from the most accurate analysis, particularly those of M. Berthollet; or the differences are such as admit of an easy explanation.

I stated in my last communication, the probability that the oxygen in the hyperoxymuriate of potash was in triple combination with the metal and oxymuriatic gas; the new facts respecting the peroxide confirm this idea. Potassium, perfectly saturated with oxygen, would probably contain six proportions; for, according to Mr. Chenevix's analysis, which is confirmed by one made in the Laboratory of the Royal Institution, by

atmosphere of heat; and his views, though not developed with precision, approach nearer to those of Mr. Dalton, than those of his cousin. But neither of these gentlemen attempted any statical expressions; and to Richter and Dalton belongs the exclusive merit of having made the doctrine practicable. As a theoretical view, other authors have a claim to it, and the earlier followers of Newton, such as Kiel, Hartley, and Marzucchi, all attempted a corpuscular chemistry, founded upon figure, weight, and attractive power of the ultimate particles of matter; but this chemistry was of no real use, and had no other foundation than in the imagination. Indeed, in my opinion, Mr. Dalton is too much of an *Atomic Philosopher;* and in making atoms arrange themselves according to his own hypothesis, he has often indulged in vain speculation; and the essential and truly useful part of his doctrine, the expression of the quantities in which bodies combine, is perfectly independent of any views respecting the ultimate nature either of matter or its elements.

Mr. E. Davy, hyperoxymuriate of potash must consist of 40·5 potassium, 32·9 oxymuriatic gas, and 45 of oxygen.*

I have mentioned, that by strongly heating the peroxide of potassium in oxymuriatic acid, all the oxygen is expelled, and a mere combination of oxymuriatic gas and potassium formed. I thought it possible, that at a low temperature, a combination might be effected, and I have reason to believe that this is the case. I made a peroxide of potassium, by heating potassium with about twice the quantity of nitre, and admitted oxymuriatic gas which was absorbed: some oxygen was expelled on the fusion of the peroxide, but a salt remained, which gave oxymuriatic gas, as well as muriatic acid, by the action of sulphuric acid.

I seems evident, that in the formation of the hyperoxymuriate of potash, one quantity of potash is decomposed by the attraction of muriatic gas to form muriate of potash; but the oxygen, instead of being set free in the nascent state, enters into combination with another portion of potash, to form a peroxide, and with oxymuriatic gas.

The proportions required for these changes may be easily deduced from the data which have been stated in the preceding pages. 5 proportions of potash, equal to 240 grains, must be decomposed to form with an equal number of proportions oxymuriatic gas equal to 164·5 grains, 5 proportions of muriate of potash equal to 367 grains; and 5 of oxygen equal to 37·5 grains, combined with one of potash, equal to 48, must unite in triple union with one of oxymuriatic gas equal to 32·9, to form one proportion, equal to 118·4 grains of hyperoxymuriate of potash.

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* [The opinion now commonly received respecting the composition of this salt, is more simple, and in accordance with the analogies of chemistry, viz., that it is a chlorate, composed of chloric acid and potash.]

III. *On the Combinations of the Metals of the Earths, with Oxygen and Oxymuriatic Gas.*

The muriates of baryta, lime, and strontia, after being a long time in a white heat, are not decomposable by any simple attractions: thus, they are not altered by dry boracic acid, though, when water is added to them, they readily afford muriatic acid and their peculiar earths.

From this circumstance, I was induced to believe that these three compounds consist merely of the peculiar metallic bases, which I have named barium, strontium, and calcium, and oxymuriatic gas; and such experiments as I have been able to make, confirm the conclusion.

When baryta, strontia, or lime, is heated in oxymuriatic gas to redness, a body precisely the same as a dry muriate is formed, and oxygen is expelled from the earth. I have never been able to effect so complé a decomposition of these earths by oxymuriatic gas, as to ascertain the quantity of oxygen produced from a given quantity of earth. But in three experiments made with great care I found that one of oxygen was evolved for every two in volume of oxymuriatic gas absorbed.

I have not yet tried the experiment of acting upon oxymuriatic gas by the bases of the alkaline earths; but I have not the least doubt that these bodies would combine directly with that substance, and form dry muriates.

In the last experiments that I made on the metallization of the earths by amalgamation, I paid particular attention to the state of the products formed, by exposing the residuum of amalgams to the air. I found that baryta formed in this way was not fusible at an intense white heat, and that strontia and lime so formed gave off no water when ignited. Baryta made from crystals of the earth, as M. Berthollet has shewn, is a fusible hydrat, and I found that this earth gave moisture when decomposed by oxymuriatic gas; and the lime, in hydrat of lime, was much more rapidly decomposed by oxymuriatic gas than quicklime, its oxygen being rapidly expelled with the water.

Some dry quicklime was heated in a retort, filled with muriatic acid gas; water was instantly formed in great abundance, and it can hardly be doubted, that this arose from the hydrogen of the acid combining with the oxygen of the lime.

As potassium so readily decomposes common salt, I thought it might possibly decompose muriate of lime, and thus afford easy means of procuring calcium. The rapidity with which muriate of lime absorbs water, and the difficulty of freeing it even by a white heat from the last portions, rendered the circumstances of the experiments unfavourable. I found, however, that by heating potassium strongly, in contact with the salt, in a retort of difficultly fusible glass, I obtained a dark coloured matter, diffused through a vitreous mass, which effervesced strongly with water. The potassium had all disappeared, and the retort had received a heat at which potassium entirely volatilizes. I had similar results with muriate of strontia, and (though less distinct, more potassium distilling off unaltered) with muriate of baryta. Either the bases of the earths were wholly or partially deprived of oxymuriatic gas in these processes, or the potassium had entered into triple combination with the muriates. I hope on a future occasion to be able to decide this point.

Combinations of muriatic acid gas with magnesia, alumine and silex, are all decomposed by heat, the acid being driven off, and the earth remaining free.* I conjectured from this circumstance, that oxymuriatic gas would not expel oxygen from these earths,† and the suspicion was confirmed by experiments. I heated magnesia, alumine, and silex to redness in oxymuriatic gas, but no change took place.

MM. Gay Lussac and Thenard have shown that baryta is capable of absorbing oxygen; and it seems likely, (as according to Mr. Chenevix's experiments, most of the earths are capable of becoming hyperoxymuriates) that peroxides of their bases must exist.

I endeavoured to combine lime with more oxygen, by heating it in hyperoxymuriate of potash, but without success, at least after this process it gave off no oxygen in combining with water. The salt, called oxymuriate of lime, made for the use of the bleachers, I found, gave off oxygen by heat, and formed muriate of lime.

From the proportions which I have given in the last Bakerian Lecture, but which were calculated from the analyses of sulphates, it follows that if the muriates of baryta, strontia, and lime, be regarded as containing one proportion of oxymuriatic gas, and one of metal, then they would consist of 71‡ barium, 46 strontium, and 21 calcium, to 32·9 of oxymuriatic gas.

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* [Chlorides of magnesium, aluminum, and silicium, have since been formed,—the first fixed and fusible, the second deliquescent and volatile, and the last liquid and volatile.]

† From some experiments of MM. Gay Lussac and Thenard, Bullet. de la Sociét. Phil. Mai, 1810, it appears that oxygen is procured by passing oxymuriatic gas over magnesia, at a high temperature, and that a muriate indecomposable by heat is formed. They attribute the presence of this oxygen to the decomposition of the acid, but according to all analogies, it must arise from the decomposition of the earth.—[*After-note by the Author.*]

‡ If Mr. James Thompson's analysis of sulphate of barytes be made

To determine how far these numbers are accurate, 50 grains of each of these muriates that had been heated to whiteness were decomposed by nitrate of silver, the precipitate was collected, washed, heated, and weighed.

The muriate of baryta, treated in this way, afforded 68 grains of horn-silver.

The muriate of strontia 85 grains.

The muriate of lime 125 grains.

From experiments to be detailed in the next section, it appears that horn-silver consists of 12 of silver to 3·9 of oxymuriatic gas, and consequently that barium should be represented by 65·1, strontium by 46·1, and calcium by 20·8.

IV. *On the Combinations of the Common Metals with Oxygen and Oxymuriatic Gas.*

In the limits which it is usual to adopt in this lecture, it will not be possible for me to give more than an outline of the numerous experiments that I have made on the combinations of oxymuriatic gas with metals; I must confine myself to a general statement of the mode of operating, and the results. I used in all cases small retorts of green glass, containing from 3 to 6 cubical inches, furnished with stopcocks. The metallic substances were introduced, the retort exhausted and filled with the gas to be acted upon, heat was applied by means of a spirit lamp, and after cooling, the results were examined, and the residual gas analysed.

All the metals that I tried, except silver, lead, nickel, cobalt, and gold, when heated, burnt in the oxymuriatic gas, and the volatile metals with flame. Arsenic, antimony, tellurium and zinc with a white flame, mercury with a red flame. Tin became ignited to whiteness, and

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the basis of calculation, sulphuric acid being estimated as 36, then the number representing barium will be about 65·5.

iron and copper to redness: tungsten and manganese to dull redness; platina was scarcely acted upon at the heat of fusion of the glass.

The product from arsenic was butter of arsenic; a dense, limpid, highly volatile fluid, a non-conductor of electricity, and of high specific gravity, and which when decomposed by water, gave oxide of arsenic and muriatic acid. That from antimony, was butter of antimony, an easily fusible and volatile solid, of the colour of horn-silver, of great density, crystallizing on cooling in hexahedral plates, and giving, by its decomposition by water, white oxide.

The product from tellurium, in its sensible qualities, resembled that from antimony, and gave, when acted on by water, white oxide.

The product from mercury was corrosive sublimate. That from zinc was similar in colour to that from antimony, but was much less volatile.

The combination of oxymuriatic gas and iron, was of a bright brown; but having a lustre approaching to the metallic, and was iridescent like the Elba iron ore. It volatilized at a moderate heat, filling the vessel with beautiful minute crystals of extraordinary splendour, and collecting in brilliant plates, the form of which I could not determine. When acted on by water, it gave red muriate of iron.

Copper formed a bright red brown substance, fusible at a heat below redness, and becoming crystalline and semi-transparent on cooling, and which gave a green fluid, and a green precipitate by the action of water.*

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* It is worth inquiry, whether the precipitate from oxymuriate of copper by water is not a hydrated submuriate, analogous in its composition to the crystallized muriate of Peru. This last I find affords muriatic acid and water by heat.

The substance from manganese was not volatile at a dull red heat; it was of a deep brown colour, and by the action of water became of a brighter brown: a muriate of manganese, which did not redden litmus remained in solution; and an insoluble matter remained of a chocolate colour.*

Tungsten afforded a deep orange sublimate, which, when decomposed by water, afforded muriatic acid, and the yellow oxide of tungsten.

Tin afforded Libavius's liquor, which gave a muriate by the action of water containing the oxide of tin, at the maximum of oxidation.

Silver and lead produced horn-silver and horn-lead,

The *resin of copper* discovered by Boyle, formed by heating copper with corrosive sublimate, probably contains only 1 proportion of oxymuriatic gas, whilst that above referred to must contain 2.

[The precipitate here alluded to, is the dichloride, or resin of copper: the chloride of copper is decomposed by heat, yielding the latter compound, which is fusible, and disengaging a portion of chlorine. The composition of the mineral from Peru, I have found to be such as the author conjectured; or as it has been since considered, an oxychloride. A specimen which I analysed, viewed as a submuriate, consisted of

73	brown oxide of copper,
16·2	muriatic acid,
10·8	water.

* When muriate of manganese is made by solution of its oxide in muriatic acid, a neutral combination is obtained, but this is decomposed by heat; muriatic gas flies off, and brown oxide of manganese remains. In this respect manganese appears as a link between the ancient metals and the newly discovered ones. Its muriate is decomposed like that of magnesia; and its oxide is the only one amongst those long known, as far as my experiments have gone, which neutralizes the acid energy of muriatic acid gas, so as to prevent it in solution from affecting vegetable blues.

[I have found that when muriate of manganese is heated in a tube with a very small orifice, a fusible chloride may be formed (vide Phil. Trans. for 1812), and that a chloride of magnesium may be obtained in a similar manner from the muriate of magnesia.]

and bismuth, butter of bismuth. The absorption of oxymuriatic gas was in the following proportions for two grains of each of the metals: for arsenic 3·6 cubical inches, for antimony 3·1, for tellurium, 2·4, for mercury 1·05,* for zinc 3·2, for iron 5·8, for tin 4, for bismuth 1·5, for copper 3·4, for lead ·9, for silver, the absorption of volume was $\frac{9}{10}$, and the increase of weight of the silver was equivalent to $\frac{6}{10}$ of a grain.†

In acting upon metallic oxides by oxymuriatic gas, I found that those of lead, silver, tin, copper, antimony, bismuth, and tellurium, were decomposed in a heat below redness, but the oxides of the volatile metals, more readily than those of the fixed ones. The oxides of cobalt and nickel were scarcely acted upon at a dull red heat. The red oxide of iron was not affected at a strong red heat, whilst the black oxide was rapidly decomposed at a much lower temperature; arsenical acid underwent no change at the greatest heat that could be given it in the glass retort, whilst the white oxide readily decomposed.

In cases where oxygen was given off, it was found exactly the same in quantity as that which had been absorbed by the metal. Thus 2 grains of red oxide of mercury absorbed $\frac{9}{10}$ of a cubical inch of oxymuriatic gas, and afforded 0·45 of oxygen.* Two grains of dark olive oxide from calomel decomposed by potash, absorbed about $\frac{94}{100}$ of oxymuriatic gas, and afforded $\frac{24}{100}$ of oxygen, and corrosive sublimate was produced in both cases.

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* The gas in these experiments was not freed from aqueous vapour, and as stopcocks of brass were used, a little gas might have been absorbed by the surface of this metal, so that the processes offer only approximations to the composition of the oxymuriates. The process on lead, tellurium, iron, antimony, copper, tin, mercury, and arsenic, were carried on in three successive days, during which the height of the mercury in the barometer varied from 30·26 inches to 30·15, and the height of that in the thermometer from 63·5 to 61 Fahrenheit.

The experiment on silver was made at the temperature of 52° Fahrenheit, and under a pressure equal to that of 29·9 inches.

† This agrees nearly with another experiment made by my brother, Mr. John Davy, in which 12 grains of silver increased to 15·9 during their conversion into horn-silver.

In the decomposition of the white oxide of zinc, oxygen was expelled exactly equal to half the volume of the oxymuriatic acid absorbed. In the case of the decomposition of the black oxide of iron, and the white oxide of arsenic, the changes that occurred were of a very beautiful kind; no oxygen was given off in either case, but butter of arsenic, and arsenical acid formed in one instance, and the ferruginous sublimate, and red oxide of iron in the other.

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* I have made two analyses of corrosive sublimate and calomel, with considerable care. I decomposed 100 grains of corrosive sublimate, by 90 grains of hydrate of potash. This afforded 79·5 grains of orange coloured oxide of mercury, 40 grains of which afforded 9·15 cubical inches of oxygen gas; the muriate of silver formed from the 100 grains was 102·5.

100 grains of calomel, decomposed by 90 grains of potash, afforded 82 grains of olive coloured oxide of mercury, of which 40 grains gave by decomposition by heat 4·8 cubical inches of oxygen. The quantity of horn-silver formed from the 100 grains was 58·75 grains.

In the second analysis, the quantity of oxide obtained from corrosive sublimate was 78·7; the quantity of muriate of silver formed was 103·4; the oxide produced from calomel weighed 83 grains; the horn-silver formed was 57½ grains. I am inclined to put most confidence in the last analysis; but the tenor of both is to shew that the quantity of oxymuriatic gas in corrosive sublimate, is exactly double that in calomel, and that the orange oxide contains twice as much oxygen as the black, the mercury being considered as the same in all. The olive colour of the oxide formed from calomel, is owing to a slight admixture of orange oxide, formed by the oxygen of the water used in precipitation; the tint I find is almost black, when a boiling solution of potash is used; and trituration with a little orange oxide brings the tint to olive. It has been stated, that the olive oxide thrown down from calomel by potash is a submuriate; but I have never been able to find a vestige of muriatic acid in it when well washed. It is not easy to obtain perfect precision in analyses of the oxides of mercury; water adheres to the oxides, which cannot be entirely driven off without the expulsion of some oxygen. In all my experiments, though the oxides had been heated to a temperature above 212, a little dew collected in the neck of the retort, so that the 40 grains must have been over-rated.

Two grains of white oxide of arsenic absorbed 0·8 of oxymuriatic gas.*

I doubt not that the same phenomena will be found to occur in other instances, in which the metal has comparatively a slight attraction only for oxymuriatic gas, and when it is susceptible of different degrees of oxydation, and in which the peroxide is used.

The only instance in which I tried to decompose a common metallic oxide, by muriatic acid, was in that of the fawn coloured oxide of tin; a compound of water, and Libavius's liquor separated.

From the proportions which may be gained in considering the volumes of oxymuriatic gas absorbed by the different metals, in their relations to the quantity of oxygen which would be required to convert them into oxides, it would appear, that in the experiments to which I have referred, either one, two, or three proportions of oxymuriatic gas combine with one of metal, and consequently, from the composition of the muriates, it will be easy to obtain the numbers representing the proportions in which these metals may be conceived to enter into other compounds.†

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* A singular instance of the tendency of the oxide of arsenic to become arsenical acid, occurs in its action on fused hydrat of potash, the water in the hydrat is rapidly decomposed, and arseniuretted hydrogen evolved, and arseniate of potash formed.

† From the experiments detailed in the note in the last page, it would appear that the number representing the proportion in which mercury combines must be about 200. That of silver, as would appear

V. *General Conclusions and Observations, illustrated by Experiments.*

All the conclusions which I ventured to draw in my last communication to the Society, will, I trust, be found to be confirmed by the whole series of these new inquiries.

Oxymuriatic gas combines with inflammable bodies, to form simple binary compounds; and in these cases, when it acts upon oxides, it either produces the expulsion of their oxygen, or causes it to enter into new combinations.

If it be said that the oxygen arises from the decomposition of the oxymuriatic gas, and not from the oxides, it may be asked, why it is always the quantity contained in the oxide; and why in some cases, as those of the peroxides of potassium and sodium, it bears no relation to the quantity of gas?

If there existed any acid matter in oxymuriatic gas, combined with oxygen, it ought to be exhibited in the fluid compound of one proportion of phosphorus, and two of oxymuriatic gas; for this, on such an assumption, should consist of muriatic acid (on the old hypothesis, free from water) and phosphorous acid; but this substance has no effect on litmus paper, and does not act under common circumstances, on fixed alkaline bases, such as dry lime or magnesia. Oxymuriatic gas, like oxygen, must be combined in large quantity with peculiar inflammable matter, to form acid matter. In its union with hydrogen, it instantly reddens the driest

from the results, page 338, about 100. The numbers of other metals may be learned from the data in the same page, but from what has been stated, these data cannot be considered as very correct.

litmus paper, though a gaseous body. Contrary to acids, it expels oxygen from protoxides, and combines with peroxides.

When potassium is burnt in oxymuriatic gas, a dry compound is obtained. If potassium combined with oxygen is employed, the whole of the oxygen is expelled, and the same compound formed. It is contrary to sound logic to say, that this exact quantity of oxygen is given off from a body not known to be compound, when we are certain of its existence in another; and all the cases are parallel.

An argument in favour of the existence of oxygen in oxymuriatic gas, may be derived by some persons from the circumstances of its formation, by the action of muriatic acid on peroxides, or on hyperoxymuriate of potash; but a minute investigation of the subject will, I doubt not, shew that the phenomena of this action are entirely consistent with the views I have brought forward. By heating muriatic acid gas in contact with dry peroxide of manganese, water I found was rapidly formed, and oxymuriatic gas produced, and the peroxide rendered brown. Now as muriatic acid gas is known to consist of oxymuriatic gas and hydrogen, there is no simple explanation of the result, except by saying that the hydrogen of the muriatic acid combined with oxygen from the peroxide to produce water.

Scheele explained the bleaching powers of the oxymuriatic gas, by supposing that it destroyed colours by combining with phlogiston. Berthollet considered it as acting by supplying oxygen. I have made an experiment, which seems to prove that the pure gas is incapable of altering vegetable colours, and that its operation in bleaching depends entirely upon its property of decomposing water, and liberating its oxygen.

I filled a glass globe containing dry powdered muriate of lime, with oxymuriatic gas. I introduced some dry paper tinged with litmus that had been just heated, into another globe containing dry muriate of lime; after some time this globe was exhausted, and then connected with the globe containing the oxymuriatic gas, and by an appropriate set of stopcocks, the paper was exposed to the action of the gas. No change of colour took place, and after two days there was scarcely a perceptible alteration.

Some similar paper dried, introduced into gas that had not been exposed to muriate of lime, was instantly rendered white.

Paper that had not been previously dried, brought in contact with dried gas, underwent the same change, but more slowly.

The hyperoxymuriates seem to owe their bleaching powers entirely to their loosely combined oxygen there is a strong tendency in the metal of those in common use, to form simple combinations with oxymuriatic gas, and the oxygen is easily expelled or attracted from them.

It is generally stated in chemical books, that oxymuriatic gas is capable of being condensed and crystallized at a low temperature; I have found by several experiments that this is not the case. The solution of oxymuriatic gas in water freezes more readily than pure water, but the pure gas dried by muriate of lime undergoes no change whatever, at a temperature of 40 below 0° of Fahrenheit. The mistake seems to have arisen from the exposure of the gas to cold in bottles containing moisture.*

I attempted to decompose boracic and phosphoric acids by oxymuriatic gas, but without success; from which it seems probable, that the attractions of boracium and phosphorus for oxygen are stronger than for oxymuriatic gas. And from the experiments I have already detailed, iron and arsenic are analogous in this respect, and probably some other metals.

Potassium, sodium, calcium, strontium, barium, zinc, mercury, tin, lead, and probably silver, antimony, and gold seem to have a stronger attraction for oxymuriatic gas than for oxygen.

I have as yet been able to make very few experiments on the combinations of the oxymuriatic compounds with each other, or with oxides. The liquor from arsenic, and that from tin, mix, producing an increase of temperature; and the phosphuretted, and the sulphuretted liquors unite with each other, and with the liquor of Libavius, but without any remarkable phenomena.

I heated lime gently in a green glass tube, and passed the phosphoric sublimate, the saturated oxymuriatic of phosphorus through it, in vapour; there was a violent action with the production of heat and light, and a gray fused mass was formed, which afforded by the action of water, muriate and phosphate of lime.

I introduced some vapour from the heated phosphoric sublimate, into an exhausted retort containing dry paper tinged with litmus: the colour slowly changed to pale red. This fact seems in favour of the idea that the substance is an acid; but as some minute quantity of aqueous vapour might have been present in the receiver, the experiment cannot be regarded as decisive: the strength of its attraction for ammonia, is perhaps likewise in favour of this opinion. All the oxymuriates that I have tried, indeed form triple compounds with this alkali; but phosphorus is expelled by a gentle heat from the other compounds of oxymuriatic gas and phosphorus with ammonia, and the substance remaining in combination is the phosphoric sublimate.

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* [The moist gas at 32° Fahrenheit, deposits a hydrat, in yellow crystals, consisting, according to Mr. Faraday's experiments, of one proportion of chlorine and of ten of water. Under a pressure of four atmospheres, the same inquirer found the dry gas to become liquid.]

VI. *Some Reflections on the Nomenclature of the Oxymuriatic Compounds.*

To call a body which is not known to contain oxygen, and which cannot contain muriatic acid, oxymuriatic acid, is contrary to the principles of that nomenclature in which it is adopted; and an alteration of it seems necessary to assist the progress of discussion, and to diffuse just ideas on the subject. If the great discoverer of this substance had signified it by any simple name, it would have been proper to have recurred to it; but, dephlogisticated marine acid is a term which can hardly be adopted in the present advanced era of the science.

After consulting some of the most eminent chemical philosophers in this country, it has been judged most proper to suggest a name founded upon one of its obvious and characteristic properties—its colour, and to call it *Chlorine*, or *Chloric* gas.*

Should it hereafter be discovered to be compound, and even to contain oxygen, this name can imply no error, and cannot necessarily require a change.

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* From χλωρος.

Most of the salts which have been called muriates, are not known to contain any muriatic acid, or any oxygen. Thus Libavius's liquor, though converted into a muriate by water, contains only tin and oxymuriatic gas, and horn-silver seems incapable of being converted into a true muriate.

I venture to propose for the compounds of oxymuriatic gas and inflammable matter, the name of their bases, with the termination *ane.* Thus argentane may signify horn-silver; stannane, Libavius's liquor; antimonane, butter of antimony; sulphurane, Dr. Thomson's sulphuretted liquor; and so on for the rest.

In cases when the proportion is one quantity of oxymuriatic gas, and one of inflammable matter, this nomenclature will be competent to express the class to which the body belongs, and its constitution. In cases when two or more proportions of inflammable matter combine with one of gas; or two or more of gas, with one of inflammable matter, it may be convenient to signify the proportions by affixing vowels before the name, when the inflammable matter predominates, and after the name, when the gas is in excess; and in the order of the alphabet, *a* signifying two, *e* three, *i* four, and so on.

The name muriatic acid, as applied to the compound of hydrogen and oxymuriatic gas, there seems to be no reason for altering. And the compounds of this body with oxides should be characterized in the usual manner, and as the other neutral salts.

Thus muriate of ammonia, and muriate of magnesia, are perfectly correct expressions.

I shall not dwell any longer at present upon this subject.—What I have advanced, I advance merely as suggestion, and principally for the purpose of calling the attention of philosophers to it.* As chemistry improves, many other alterations will be necessary; and it is to be hoped that whenever they take place, they will be made

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* It may be conceived that a name may be found for the oxymuriatic gas in some modification of its present appellation which may harmonize with the new views, and which may yet signify its relation to the muriatic acid, such as demuriatic gas, or oxymuric gas; but in this case it would be necessary to call the muriatic acid, hydrogenated muriatic acid, or hydromuriatic acid; and the salts which contain it hydrogenated muriates or hydromuriates; and on such a plan, the compounds of oxymuriatic gas must be called demuriates or oxymuriates, which I conceive would create more complexity and difficulty in unfolding just ideas on this department of chemical knowledge than the methods which I have ventured to propose. It may however be right, considering the infant state of the investigation, to suspend, for a time, the adoption of any new terms for these compounds. It is possible that oxymuriatic gas may be compound, and that this body and oxygen may contain some common principle; but at present we have no more right to say that oxymuriatic gas contains oxygen than to say that tin contains hydrogen; and names should express things and not opinions; and till a body is decompounded, it should be considered as simple.

In the last number of Mr. Nicholson's Journal, which appeared February 1st, whilst this sheet was correcting for press, I have seen an ingenious paper, by Mr. Murray, of Edinburgh, in which he has attempted to shew, that oxymuriatic gas contains oxygen. His methods are, by detonating oxymuriatic gas in excess, with a mixture of hydrogen, and gaseous oxide of carbon, when he *supposes* carbonic acid is formed; and by mixing oxymuriatic gas in excess, with sulphuretted hydrogen, when he *supposes* sulphuric acid, or sulphurous acid is formed. In some experiments, in which my brother, Mr. John Davy, was so good as to cooperate, made over boiled mercury, we found, that 7 parts of hydrogen, 8 parts of gaseous oxide of carbon, and 20 parts of oxymuriatic gas, exploded by the electrical spark, diminished to about 30 measures; and calomel was formed on the sides of the tube. On adding dry ammonia in excess, and exposing the remainder to water, a gas remained, which equalled more than 9 measures, and which was gaseous oxide of carbon, with no more impurity than might be expected from the air in the gases, and the nitrogen expelled from the ammonia; so that the oxygen in Mr. Murray's carbonic acid, it seems, was obtained from *water*, or from the carbonic oxide. Sulphuretted hydrogen, added over dry mercury, to oxymuriatic gas in excess, inflamed in two or three experiments;

independent of all speculative views, and that new names will be derived from some simple and invariable property, and that mere arbitrary designations will be employed, to signify the class to which compounds or simple bodies belong.

muriatic acid gas containing the vapour of oxymuriate of sulphur, was formed, which, when neutralized by ammonia, gave muriate of ammonia, and a combination of ammonia, and oxymuriate of sulphur.

When a mixture of oxymuriatic gas in excess, and sulphuretted hydrogen, was suffered to pass into the atmosphere, the smell was that of oxymuriate of sulphur; there was not the slightest indication of the presence of any sulphuric or sulphurous acid. If Mr. Murray had used ammonia, instead of water, for analyzing his results, I do not think he would have concluded, that oxymuriatic gas is capable of decomposition by such methods.

I shall not, at present, enter upon a detail of other experiments which I have made on this subject, in co-operation with my brother, as it is his intention to refer to them, in an answer to Mr. Murray's paper.*

I shall conclude, by saying, that this ingenious chemist has mistaken my views, in supposing them hypothetical; I merely state what I have seen, and what I have found. There *may* be oxygen in oxymuriatic gas; but I can find none. I repeated Mr. Murray's experiments with great interest; and their results, when *water* is excluded, entirely confirm all my ideas on the subject, and afford no support to the hypothetical ideas, which he has laboured so zealously to defend.

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* [The controversy relative to the nature of oxymuriatic acid, of which the paper above alluded to formed a part, may be found in Nicholson's Journal, Vols. 28, 29, 30, 31, 32. 34.—The principal errors which the ingenious opponent of the new doctrine fell into, arose from two new compounds, phosgene gas and euchlorine, which were discovered in the course of this inquiry, having acted in his experiments without his having been aware of their presence.]

IX.

ON A COMBINATION OF OXYMURIATIC GAS AND OXYGEN GAS.*

I SHALL beg permission to lay before the Society the account of some experiments on a compound of oxymuriatic gas and oxygen gas, which, I trust, will be found to illustrate an interesting branch of chemical inquiry, and which offer some extraordinary and novel results.

I was led to make these experiments in consequence of the difference between the properties of oxymuriatic gas prepared in different modes. It would occupy a great length of time to state the whole progress of this investigation. It will, I conceive, be more interesting that I should immediately refer to the facts; most of which have been witnessed by members of this body, belonging to the Committee of Chemistry of the Royal Institution.

The oxymuriatic gas prepared from manganese, either by mixing it with a muriate and acting upon it by sulphuric acid, or by mixing it with muriatic acid, is when the oxide of manganese is pure, and, whether collected over water or mercury, uniform in its pro-

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[* From the Phil. Trans. for 1811. Read before the Royal Society, February 21st, 1811.]

perties; its colour is a pale yellowish green; water takes up about twice its volume; and scarcely gains any colour; the metals burn in it readily; it combines with hydrogen without any deposition of moisture: it does not act on nitrous gas or muriatic acid, or carbonic oxide, or sulphurous gases, when they have been carefully dried. It is the substance which I employed in all the experiments on the combinations of oxymuriatic gas, described in my last two papers.

The gas produced by the action of muriatic acid on the salts which have been called hyperoxymuriates, on the contrary, differs very much in its properties, according as the manner in which it is prepared and collected is different.

When much acid is employed to a small quantity of salt, and the gas is collected over water, the water becomes tinged of a lemon colour; but the gas collected is the same as that procured from manganese.

When the gas is collected over mercury, and is procured from a weak acid, and from a great excess of salt, by a low heat, its colour is a dense tint of brilliant yellow green, and it possesses properties entirely different from the gas collected over water.

It sometimes explodes during the time of its transfer from one vessel to another, producing heat and light, with an expansion of volume; and it may be always made to explode by a very gentle heat, often by that of the hand.*

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* My brother, Mr. J. Davy, from whom I receive constant and able assistance in all my chemical inquiries, had several times observed explosions, in transferring the gas from hyperoxymuriate of potash, over mercury, and he was inclined to attribute the phenomenon to the combustion of a thin film of mercury, in contact with a globule of gas. I several times endeavoured to produce the effect, but without success, till an acid was employed for the preparation of the gas, so diluted as

It is a compound of oxymuriatic gas and oxygen, mixed with some oxymuriatic gas. This is proved by the results of its spontaneous explosion. It gives off, in this process, from $\frac{1}{6}$ to $\frac{2}{5}$ its volume of oxygen, loses its vivid colour, and becomes common oxymuriatic gas.

I attempted to obtain the explosive gas in a pure form, by applying heat to a solution of it in water; but in this case, there was a partial decomposition; and some oxygen was disengaged, and some oxymuriatic gas formed. Finding that in the cases when it was most pure, it scarcely acted upon mercury, I attempted to separate the oxymuriatic gas with which it is mixed, by agitation in a tube with this metal; corrosive sublimate formed, and an elastic fluid was obtained, which was almost entirely absorbed by $\frac{1}{4}$ of its volume of water.

This gas in its pure form is so easily decomposable, that it is dangerous to operate upon considerable quantities.

In one set of experiments upon it, a jar of strong glass, containing 40 cubical inches, exploded in my hands with a loud report, producing light; the vessel was broken, and fragments of it were thrown to a considerable distance.

I analysed a portion of this gas, by causing it to explode over mercury in a curved glass tube, by the heat of a spirit lamp.

The oxymuriatic gas formed, was absorbed by water; the oxygen was found to be pure, by the test of nitrous gas.

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not to afford it without the assistance of heat. The change of colour and expansion of volume, when the effect took place, immediately convinced me, that it was owing to a decomposition of the gas.

50 parts of the detonating gas, by decomposition, expanded so as to become 60 parts. The oxygen, remaining after the absorption of the oxymuriatic gas, was about 20 parts. Several other experiments were made, with similar results. So that it may be inferred, that it consists of 2 in volume of oxymuriatic gas, and 1 in volume of oxygen; and the oxygen in the gas is condensed to half its volume: circumstances conformable to the laws of combination of gaseous fluids, so ably illustrated by M. Gay Lussac, and to the theory of definite proportions.

I have stated on a former occasion, that approximations to the numbers representing the proportions in which oxygen and oxymuriatic gas combine, are found in 7·5 and 32·9. And this compound gas contains nearly these quantities.*

The smell of the pure explosive gas somewhat resembles that of burnt sugar, mixed with the peculiar smell of oxymuriatic gas. Water appeared to take up eight or ten times its volume; but the experiment was made over mercury, which might occasion an error, though it did not seem to act on the fluid. The water became of a tint approaching to orange.

When the explosive gas was detonated with hydrogen, equal to twice its volume, there was a great absorption, to more than $\frac{1}{3}$, and solution of muriatic acid was formed; when the explosive gas was in excess, oxygen was always expelled, a fact demonstrating the stronger attraction of hydrogen for oxymuriatic gas than for oxygen.

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* In page 245 of the Phil. Trans. for 1810, [page 298] I have mentioned that the specific gravity of oxymuriatic gas, is between 74 and 75 grains per 100 cubical inches. The gas that I weighed was collected over water and procured from hyperoxymuriate of potash, and at that time I conceived, that this elastic fluid did not differ from the oxymuriatic gas from manganese, except in being purer. It probably contained some of the new gas; for I find that the specific gravity of pure oxymuriatic gas from manganese, and muriatic acid is to that of common air, as 244 to 100. Taking this estimation, the specific gravity of the new gas will be about 238, and the number representing the proportion in which oxymuriatic gas combines, from this estimation, will be rather higher than is stated above.

I have said that mercury has no action upon this gas in its purest form at common temperatures.* Copper and antimony, which so readily burn in oxymuriatic gas, did not act upon the explosive gas in the cold: and when they were introduced into it, being heated, it was instantly decomposed, and its oxygen set free; and the metals burnt in the oxymuriatic gas.

When sulphur was introduced into it, there was at first no action, but an explosion soon took place; and the peculiar smell of oxymuriate of sulphur was perceived.

Phosphorus produced a brilliant explosion, by contact with it in the cold, and there was produced phosphoric acid and solid oxymuriate of phosphorus.

Arsenic introduced into it did not inflame; the gas was made to explode, when the metal burnt with great brilliancy in the oxymuriatic gas.

Iron wire introduced into it did not burn, till it was heated so as to produce an explosion, when it burnt with a most brilliant light in the decomposed gas.

Charcoal introduced in it ignited, produced a brilliant flash of light, and burnt with a dull red light, doubtless owing to its action upon the oxygen mixed with the oxymuriatic gas.

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* [Later experiments have not confirmed this—at least in an absolute manner, as M. Soubeiran's in Ann. de Chimie et Physique, tom. 48, and my own in the Edinburgh New Philosophical Journal, vol. xvii.—with some other facts giving rise to the idea that this gas is a mixture of chlorine, and of perchlorine, a compound discovered by the author in 1815, and which will be found described in a subsequent paper.]

It produced dense red fumes when mixed with nitrous gas, and there was an absorption of volume.

When it was mixed with muriatic acid gas, there was a gradual diminution of volume. By the application of heat the absorption was rapid, oxymuriatic gas was formed, and a dew appeared on the sides of the vessel.

These experiments enable us to explain the contradictory accounts that have been given by different authors of the properties of oxymuriatic gas.

That the explosive compound has not been collected before, is owing to the circumstance of water having been used for receiving the products from hyperoxymuriate of potash, and unless the water is highly saturated with the explosive gas, nothing but oxymuriatic gas is obtained; or to the circumstance of too dense an acid having been employed.

This substance produces the phenomena which Mr. Chenevix, in his able paper on oxymuriatic acid, referred to the hyperoxygenised muriatic acid; and they prove the truth of his ideas respecting the possible existence of a compound of oxymuriatic gas, and oxygen in a separate state.

The explosions produced in attempts to procure the products of hyperoxymuriate of potash by acids are evidently owing to the decomposition of this new and extraordinary substance.

All the conclusions which I have ventured to make respecting the undecompounded nature of oxymuriatic gas, are, I conceive, entirely confirmed by these new facts.

If oxymuriatic gas contained oxygen, it is not easy to conceive, why oxygen should be afforded by this new compound to muriatic gas, which must already contain oxygen in intimate union. Though on the idea of muriatic acid being a compound of hydrogen and oxymuriatic gas, the phenomena are such as might be expected.

If the power of bodies to burn in oxymuriatic gas depended upon the presence of oxygen, they all ought to burn with much more energy in the new compound; but copper and antimony, and mercury, and arsenic, and iron, and sulphur have no action upon it, till it is decomposed; and they act then according to their relative attractions on the oxygen, or on the oxymuriatic gas.

There is a simple experiment which illustrates this idea. Let a glass vessel containing brass foil be exhausted, and the new gas admitted, no action will take place; throw in a little nitrous gas, a rapid decomposition occurs, and the metal burns with great brilliancy.

Supposing oxygen and oxymuriatic gas to belong to the same class of bodies; the attraction between them might be conceived very weak, as it is found to be, and they are easily separated from each other, and made repulsive by a very low degree of heat.

The most vivid effects of combustion known, are those produced by the condensation of oxygen or oxymuriatic gas; but in this instance, a violent explosion with heat and light are produced by their separation, and expansion, a perfectly novel circumstance in chemical philosophy.

This compound destroys dry vegetable colours, but first gives them a tint of red. This and its considerable absorbability by water would incline one to adopt Mr.

Chenevix's idea that it approaches to an acid in its nature. It is probably combined with the peroxide of potassium in the hyperoxymuriate.

That oxymuriatic gas and oxygen combine and separate from each other with such peculiar phenomena, appears strongly in favour of the idea of their being distinct, though analogous species of matter. It is certainly possible to defend the hypothesis that oxymuriatic gas consists of oxygen united to an unknown basis; but it would be possible likewise to defend the speculation that it contains hydrogen.

Like oxygen it has not yet been decomposed; and I some time ago made an experiment, which, like most of the others I have brought forward, is very adverse to the idea of its containing oxygen.

I passed the solid oxymuriate of phosphorus in vapour, and oxygen gas together, through a green glass tube heated to redness.

A decomposition took place, and phosphoric acid was formed, and oxymuriatic gas was expelled.

Now, if oxygen existed in the oxymuriate of phosphorus, there is no reason why this change should take place. On the idea of oxymuriatic gas being undecompounded, it is easily explained. Oxygen is known to have a stronger attraction for phosphorus than oxymuriatic gas has, and consequently ought to expel it from this combination.

As the new compound in its purest form is possessed of a bright yellow green colour, it may be expedient to designate it by a name expressive of this circumstance, and its relation to oxymuriatic gas. As I have named that elastic fluid Chlorine, so I venture to propose for this substance the name Euchlorine, or Euchloric gas, from ευ and χλωρος. The point of nomenclature I am not, however, inclined to dwell upon. I shall be content to adopt any name that may be considered as most appropriate by the able chemical philosophers attached to this Society.

X.

ON SOME COMBINATIONS OF PHOSPHORUS AND SULPHUR, AND ON SOME OTHER SUBJECTS OF CHEMICAL INQUIRY.*

I. *Introduction.*

In this paper, I shall do myself the honour of laying before the Society the result of some experiments on phosphorus and sulphur, which establish the existence of some new compounds, and which offer decided evidences in favour of an idea that has been for some time prevalent amongst many enlightened chemists, and which I have defended in former papers published in the Philosophical Transactions; namely, that bodies unite in definite proportions, and that there is a relation between the quantities in which the same element unites with different elements.

I shall not enter into a minute detail of the methods of experimenting that I employed; I shall confine myself to general statements of the facts. The common manipulations of chemistry are now too well known to require any new illustrations: and to dwell upon familiar operations, would be to occupy unnecessarily and tediously the time of this learned body.

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* [From the Phil. Trans. for 1812. Read before the Royal Society, June 18, 1812.]

II. *Of some Combinations of Phosphorus.*

In a paper read before the Royal Society in 1810,* I have described the mutual action of phosphorus and oxymuriatic gas, or chlorine. I have noticed two compounds which appear to be distinct and peculiar bodies, formed by the union of the gas and the inflammable substance. One is solid, white, and crystalline in its appearance; easily volatile, and capable of forming a fixed infusible substance by uniting with ammonia. The other is fluid, limpid as water, and, as I have since found, of specific gravity 1·45; it produces dense fumes by acting upon the water of the atmosphere, and when exposed to the atmosphere gradually disappears, leaving no residuum.

The composition of the white sublimate is very easily ascertained by synthetical experiments, such as I have described on a former occasion in the Transactions. By employing chlorine dried by muriate of lime, in great excess, and making the experiments in exhausted vessels, and admitting solution of chlorine to ascertain the quantity of gas absorbed, I have ascertained that 3 grains of phosphorus unite with about 20 grains of chlorine to form the sublimate.

If the phosphorus be in great excess in the experiment of its combustion in chlorine, some of the liquor is formed with the sublimate; but to obtain it in considerable quantities, phosphorus should be passed in vapour through heated powdered corrosive sublimate. A bent glass tube may be used for the process, and the liquor condensed in a cold vessel connected with the tube.

I have not been able to determine its composition by synthetical experiments; but by pouring it gradually into water, suffering the water to become cool after each addition of the liquor, and then precipitating the solution by solution of nitrate of silver, I have ascertained the quantity of chlorine and of phosphorus it contains. 13·6 grains, treated in this way, afforded 43 grains of horn-silver.

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* [Page 286 of this Vol.]

It is evident from this analysis, compared with the result of the synthetical experiments on the sublimate, that the quantity of phosphorus being the same, the sublimate contains double as much chlorine as the liquor.

When phosphorus is heated in the liquor, a portion is dissolved, and it then when exposed to the atmosphere leaves a film of phosphorus, which when the liquor is thrown on paper usually inflames: a substance of this kind was first procured by MM. Gay Lussac and Thenard, by distilling phosphorus and calomel together; and it may be produced in the experiment with corrosive sublimate, if sufficient heat be used to sublime the phosphorus, or if there be not an excess of the corrosive sublimate. I have made no experiments in order to ascertain the quantity of phosphorus the liquor will dissolve.

When the white sublimate is made to act upon water, it dissolves in it, producing much heat. The solution evaporated affords a thick liquid, which is a solution of pure phosphoric acid, or a hydrat of phosphoric acid.

When the liquor is treated with water in the same way, it furnishes likewise a thick fluid of the consistence of syrup, which crystallizes slowly by cooling, and forms transparent parallelopipedons.

This substance has very singular properties: when it is heated pretty strongly in the air, it takes fire and burns brilliantly, emitting at the same time globules of gas, that inflame at the surface of the liquid. This substance may be called *hydrophosphorous* acid; for it consists of pure phosphorous acid and water. This is proved by the action of ammoniacal gas upon it; when it is heated in contact with ammonia, water is expelled and phosphite of ammonia formed; and it is likewise shewn by the results of its decomposition in close vessels, which are phosphoric acid and a peculiar compound of phosphorus and hydrogen.

Ten parts in weight of the crystalline acid I found produced about 8·5 parts of solid phosphoric acid, and the elastic product must of course have formed the remainder of the weight, allowing for a small quantity of the substance not decomposed.

The peculiar gas is not spontaneously inflammable; but explodes when mixed with air, and heated to a temperature rather below 212°.

Its specific gravity appeared from an experiment in which a small quantity of it only was weighed, to be to that of air nearly as 87 to 100. Water absorbed about one-eighth of its volume of this gas. Its smell was disagreeable, but not nearly so fetid as that of common phosphuretted hydrogen.

When it was detonated with oxygen, it was found that three of it in volume absorbed more than five in volume of oxygen, and a little phosphorus was precipitated.

When potassium was heated in contact with it, its volume increased rapidly till it became double, and then no further effect was produced. The potassium was partly converted into a substance having all the characters of phosphuret of potassium; and the residual gas absorbed the same quantity of oxygen by detonation as pure hydrogen. When sulphur was sublimed in the gas over mercury, the volume was likewise doubled; a compound of phosphorus and sulphur was formed, and the elastic fluid produced had all the characters of sul phuretted hydrogen.

It appears from these experiments, that the peculiar gas consists of 4·5 of hydrogen in weight to 22·5 of phosphorus; and its composition being known, it is easy to determine the composition of the hydrophosphorous acid, and likewise the quantity of oxygen required by a given quantity of phosphorous acid to be converted into phosphoric acid; for, for every volume of gas disengaged, a volume of oxygen must have been fixed in the phosphoric acid.

And calculating for 174 grains, 30 parts of oxygen must be fixed in the 150 parts of phosphoric acid, and 20 parts of phosphorus disengaged in combination with 4 parts of hydrogen; and on the idea of representing the proportions in which bodies combine by numbers, if hydrogen be considered as unity and water as composed of 2 proportions of hydrogen, 2, and one of oxygen 15,* phosphorus will be represented by 20.

When the compounds of chlorine and phosphorus are acted on by a small quantity of water, muriatic acid gas is disengaged with violent ebullition, the water is decomposed, and it is evident that for every volume of hydrogen disengaged in combination with the chlorine, half a volume of oxygen must be combined with the phosphorus; and the products of the mutual decomposition of water, and the phosphoric compounds of chlorine are merely the phosphoric acid from the sublimate and the phosphorous acid from the liquor, and muriatic acid gas; so that the quantity of phosphorus being the same, it is evident that phosphoric acid must contain twice as much oxygen as phosphorous acid, which harmonizes with the results of the decomposition of hydrophosphorous acid. For supposing water to be composed of 2 proportions of hydrogen, and one of oxygen, and the number representing it 17; then 174 parts of hydrophosphorous acid· must consist of 2 proportions; 34 parts of water, and four proportions of phosphorous acid, containing 80 of phosphorus and 60 of oxygen; and three proportions of phosphoric acid must be formed, containing three proportions of phosphorus 60, and 6 proportions of oxygen 90, making 150.

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* Supposing 100 cubical inches of the gas to weigh 27 grains.——27 —4·5 the weight of 200 cubical inches of hydrogen = 22·5 grains.

This mode of estimation is the same as that I have adopted on a former occasion, except that the number representing oxygen is doubled to avoid a fractional part.

It is scarcely possible to imagine more perfect demonstrations of the laws of definite combination, than those furnished in the mutual action of water and the phosphoric compounds. No products are formed except the new combinations; neither oxygen, hydrogen, chlorine, nor phosphorus is disengaged, and therefore the ratio in which any two of them combine being known, the ratios in which the rest combine, in these cases, may be determined by calculation.

I converted phosphorus into phosphoric acid, by burning it in a great excess of oxygen gas over mercury in a curved glass tube, and heated the product strongly. I found in several processes of this kind, that for every grain of phosphorus consumed, four cubical inches and a half of oxygen gas were absorbed; which gives phosphoric acid as composed of 20 of phosphorus to 30·6 of oxygen;* a result as near as can be expected to the results of the experiments on the sublimate and the hydrophosphorous acid.

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* [Vide the next paper for the author's later and more accurate researches on this subject.]

Unless the product of the combustion of phosphorus is strongly heated in oxygen, the quantity of oxygen absorbed is less, so that it is probable that phosphorous acid is formed, as well as phosphoric acid.

Phosphorous acid is usually described, in chemical authors, as a fluid body, and as formed by the slow combustion of phosphorus in the air; but the liquid so procured is, I find, a solution of a mixture of phosphorous and phosphoric acids. And the vapour arising from phosphorus in the air at common temperatures, is a combination of phosphorous acid and the aqueous vapour in the air, and is not, I find, perceived in air artificially dried.

In this case, the phosphorus becomes covered with a white film, which appears to be pure phosphorous acid, and it soon ceases to shine.

A solid acid, volatile at a moderate degree of heat, may be produced by burning phosphorus in very rare air, and this seems to be phosphorous acid free from water; but some phosphoric acid, and some yellow oxide of phosphorus, are always formed at the same time.

The peculiar gas differs exceedingly from phosphuretted hydrogen formed by the action of earths and alkalies and phosphorus upon water; for this last gas is spontaneously inflammable, and its specific gravity is seldom more than half as great, and it does not afford more than 1·5 its volume of hydrogen when decomposed by potassium; it differs in its qualities in different cases, and probably consists of different mixtures of hydrogen with a peculiar gas, consisting of 2 parts of hydrogen and 20 of phosphorus; or it must contain several proportions of hydrogen to one of phosphorus.

I venture to propose the name *hydrophosphoric* gas for the new gas; and according to the principles of nomenclature I have proposed in the last Bakerian Lecture, the liquor containing 20 of phosphorus to 67 of chlorine may be called *phosphorane*, and the sublimate *phosphorana*.

III. *Of some Combinations of Sulphur.*

I have shewn, in a paper published in the Philosophical Transactions for 1810, that sulphuretted hydrogen is formed by the solution of sulphur in hydrogen, and I have supposed that sulphurous acid in like manner, is constituted by a solution of sulphur in oxygen. There is always a little condensation of volume in experiments on the combustion of sulphur in oxygen; but this may fairly be attributed to some hydrogen loosely combined in the sulphur; and to the production of a little sulphuric acid by the mutual action of hydrogen, oxygen, and sulphur.

It is only necessary, if these data be allowed, to know the difference between the specific gravity of sulphurous acid gas and oxygen, and sulphuretted hydrogen and hydrogen, to determine their composition.

In the Philosophical Transactions for 1810, page 254,* I have somewhat under-rated the weights of sulphuretted hydrogen and sulphurous acid gases: for I have since found, that the cubical inch measures, employed for ascertaining the volumes of gas weighed, were not correct. From experiments which I think may be depended upon, as the weights of the gases were merely compared with those of equal volumes of common air, I found that 100 cubical inches of sulphurous acid gas weighed 68 grains at mean temperature and pressure, and 100 cubical inches of sulphuretted hydrogen 36·5 grains, and the last result agrees very nearly with one given by MM. Gay Lussac and Thenard, and one gained by my brother Mr. John Davy.

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* [Page 308 of this Vol.]

If 34, the weight of 100 cubical inches of oxygen gas, be subtracted from 68, it will appear that sulphurous acid consists of equal weights of sulphur and oxygen, an estimation which agrees very nearly with one given by M. Berzelius; and if 2·27, the weight of 100 cubical inches of hydrogen, be subtracted from 36·5, the remainder 34·23 will be the quantity of sulphur in the gas; and the number representing sulphur may be stated as 30; and sulphurous acid as composed of one proportion of sulphur 30, and two of oxygen 30; and sulphuretted hydrogen as composed of one proportion of sulphur, and two of hydrogen.

From the experiments of M. Gay Lussac, it appears that sulphuric acid decomposed by heat affords one volume of oxygen to two of sulphurous acid: from this it would appear to be composed of one proportion of sulphur to three of oxygen. I have endeavoured, in several trials by common heat and by electricity, to combine sulphurous acid gas with oxygen, so as to form a sulphuric acid free from water, but without success; and it is probable, that three proportions of oxygen cannot be combined with one proportion of sulphur, except by the intermedium of water. Mr. Dalton has supposed, that there is a solid sulphuric acid formed by the action of sulphurous acid gas upon nitrous acid gas. But I find, that when dried sulphurous acid gas and nitrous acid gas are mixed together, there is no action;

but by introducing the vapour of water, they form together a solid crystalline hydrat; which when thrown into water gives off nitrous gas, and forms a solution of sulphuric acid.

I have referred, in the Philosophical Transactions, to the combination of chlorine and sulphur. I have been able to form no compound of these bodies, which does not deposit sulphur by the action of water. When sulphur is saturated with chlorine, as in Dr. Thomson's sulphuretted liquor, it appears to contain, from my experiments, only 67 of chlorine to 30 of sulphur.

IV. *Some General Observations.*

It is a fact worthy of notice, that phosphoric and sulphuric acids should contain the same quantity of oxygen to the same quantity of inflammable matter; and yet that the oxygen should be combined in them, with such different degrees of affinity. Phosphorous acid has a great tendency to unite with oxygen, and absorbs it even from water: and sulphurous acid can only retain it when water is present.

The relation of water to the composition of many bodies has already occupied the attention of some distinguished chemists, and is well worthy of being further studied; most of the substances obtained by precipitation from aqueous solutions are, I find, compounds of water.

Thus zircona, magnesia, silica, when precipitated and dried at 212° still contain definite proportions of water. And many of the substances which have been considered as metallic oxides, that I have examined, obtained from solutions, agree in this respect; and their colours and other properties are materially influenced by this combined water.

I shall give an instance. The substance which has been called the white oxide of manganese is a compound of water and the protoxide of manganese, and when heated strongly, it gives off its water and becomes a dark olive oxide.

It has been often suspected, that the contraction of volume produced in the pure earths by heat, is owing to the expulsion of water combined with them. The following fact seems to confirm this suspicion, and offers a curious phenomenon.

Zircona, precipitated from its solution in muriatic acid by an alkali, and dried at a temperature below 300°, appears as a white powder, so soft as not to scratch glass. When heated to 700° or 800°, water is suddenly expelled from it, and notwithstanding the quantity of vapour formed, it becomes at the moment red hot. After the process, it is found harsh to the feel, has gained a tint of gray, its parts cohere together, and it is become so hard as to scratch quartz.

XI.

NEW EXPERIMENTS ON SOME OF THE COMBINATIONS OF PHOSPHORUS.*

In a paper published in the Transactions of the Royal Society for 1812, I have detailed a number of experiments on phosphorus, from which I deduced the composition of some of its compounds with oxygen, with hydrogen and with chlorine. Since the appearance of this paper, various researches have been brought forward on the same subject, in which some results, differing very much from each other, and from mine are stated. I ventured to conclude that the phosphoric acid contained double the quantity of oxygen to that in the phosphorous acid; and that phosphoric acid contained about $\frac{3}{5}$ of its weight of oxygen.

M. Berzelius considers the oxygen in phosphoric acid to be 128·17, and M. Dulong, 124·5, the phosphorus being 100. M. Dulong, and M. Berzelius, suppose the quantity of oxygen in phosphorous acid to be to that in phosphoric acid as 3 to 5.

The motive which immediately induced me to resume the inquiry respecting the phosphoric combinations, was M. Dulong's paper.* This ingenious chemist has not only endeavoured to establish new proportions in the known compounds of phosphorus, but has likewise attempted to prove the existence of two new acids of phosphorus; and has denied several facts which I considered as sufficiently established.

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* [From the Phil. Trans. for 1818. Read before the Royal Society, April 9th, 1818. This paper is inserted out of its place in relation to time, on account of its connection with the preceding.]

The details which I have to lay before the Society in the following pages, will serve to connect and fix, I hope, with tolerable accuracy, the proportional number or equivalent of phosphorus, and at the same time will show the truth of the general series of proportions that I assigned to its compounds. In a case where my conclusions differ so materially from those of MM. Berzelius and Dulong, it may be supposed that I have not adopted them without considerable caution; and I have preferred my own results to theirs, only because they have been confirmed by minute and repeated experiments.

I was certain from various experiments made both long ago and recently, and the results of which had been confirmed by Mr. Brande, that the proportion of oxygen, which M. Dulong assigns to phosphoric acid, is considerably smaller than that denoted by the combustion of small quantities of phosphorus in oxygen gas. I knew that minute portions of phosphuretted hydrogen were separated from phosphorus by voltaic electricity; and it occurred to me as possible, that water might be formed in the combustion of phosphorus, and separated from the phosphoric acid when it entered into saline and metallic combinations. To ascertain if this were

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[* Vide Mem. d'Arcueil, tom. iii., for M. Dulong's paper, and Annal. de Chimie, et de Physique, tom. ii. p. 141, for an extract from it, containing his principal results; M. Berzelius' Memoir referred to, is in the same volume.]

the case, I passed phosphorus to saturation through red hot lime in a green glass tube connected with a mercurio-pneumatic apparatus; the combination took place with vivid ignition; but no elastic fluid was produced. A portion of the phosphuret of lime formed, was introduced into a tray of platinum, and heated in a glass retort filled with oxygen gas; the phosphuret of lime burnt brilliantly, and became partly converted into *phosphate* of *lime;* but on restoring the original temperature of the retort, there was no appearance of *vapour* or of *moisture.*

Having examined the phosphate of lime formed, in this operation, and satisfied myself that it was the same as that formed by other methods, it became evident that there were no sources of error in the experiments on the combustion of phosphorus in oxygen gas, arising from the formation or separation of water; and the only circumstance which could be urged against the accuracy of processes on this combustion was the small quantity of materials* on which they had been made.

The vividness and rapidity of the combustion of phosphorus, renders it impossible to burn considerable quantities of phosphorus in the common way in glass vessels. Phosphuret of lime burns much more slowly and less intensely. I endeavoured to ascertain the quantity of oxygen absorbed by a given weight of phosphorus converted into phosphuret of lime; but the experiment did not succeed. Though the phosphuret of lime was in fine powder, and distributed over a large surface, yet the phosphate of lime which formed and fused on the exterior, defended the interior of the phosphuret from the action of the oxygen, and prevented its combustion.

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* A source of error might be suspected in carbon combined with phosphorus; but I have been convinced by experiments made on the action of chlorine on the phosphorus I employed, that it contained no appreciable quantity of carbon. I suspect that what is often taken for the carburet of phosphorus, is in reality a red oxide.

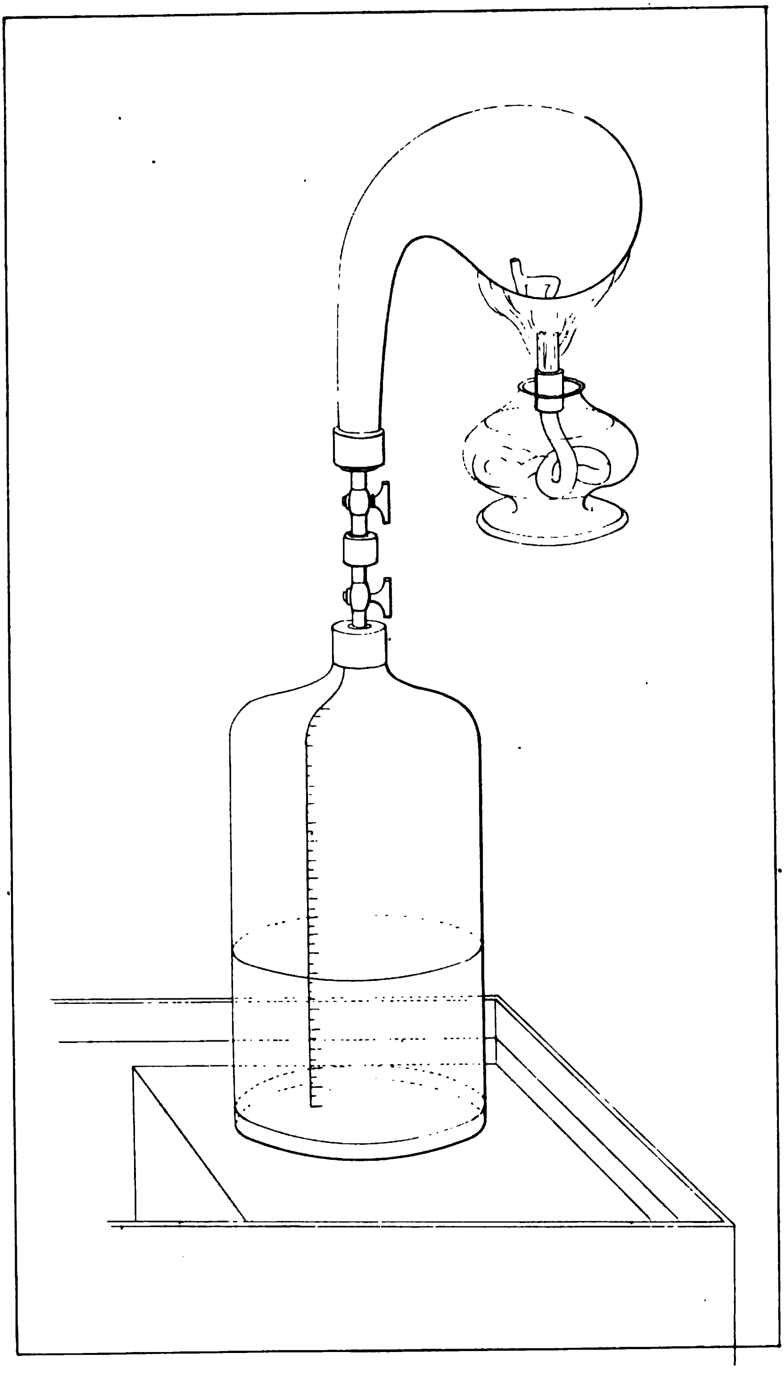
After several unsuccessful trials to convert considerable quantities of phosphorus into phosphoric acid by combinations containing oxygen, I at last thought of a very simple mode of burning phosphorus, which answered perfectly.

Phosphorus requires a considerable heat for its volatilization. By inclosing it in a small tube, so constructed that the phosphorus can burn in vapour only, from the aperture of the tube large quantities of it may be burnt by the heat of a spirit lamp in a retort filled with oxygen, and the absorption of oxygen and the quantity of phosphoric acid formed may be minutely ascertained.

The accompanying sketch will give an idea of the apparatus. The neck of the little curved tube, or small distilling retort, after the phosphorus is introduced, is drawn out, and an aperture left of about one-tenth of an inch; it should not be smaller, or it becomes choaked by the phosphoric acid formed. Regulating the heat by raising or lowering the spirit lamp, the combustion may be carried on slowly, or rapidly, at pleasure.

Operating in this way, I have often burnt from 5 to 10 grains of phosphorus without any accident, and ascertained exactly the quantity of oxygen absorbed; there is only one source of error—a quantity of phosphorus remains in the upper part of the tube, which cannot be burnt except by a greater heat than the retort will bear; and it is difficult to ascertain the precise weight of this, as the tube always unites with some

PLATE 5.



phosphoric acid when it is red hot at its mouth; but this can only be a trifling source of error.

In these experiments, and in all the others detailed in this paper, I received much useful assistance from Mr. Faraday, of the Royal Institution; and much of their value, if they shall be found to possess any, will be owing to his accuracy and steadiness of manipulation.

Experiment I.

Six grains of phosphorus. The small tube with the phosphorus weighed before the combustion 56·5 grains: after the combustion 50·9; so that it had increased $\frac{4}{10}$; and this increase was in a great measure from phosphorus that had escaped combustion; and when this was burnt out by a strong red heat, the increase of the weight of the tube was under $\frac{1}{10}$; so that at least 5·9 phosphorus had been converted into acid; 23·5 cubical inches of oxygen were absorbed; thermometer being at 46° Fahrenheit; barometer 29·6 inches.

Experiment II.

Ten grains of phosphorus. The glass tube containing the phosphorus weighed 103·1 grains; after the experiments 95·6; but much phosphorus remained unconsumed. After the tube had been heated to redness it weighed 94 grains; so that at least 8·4 grains of phosphorus were consumed in the first process. The absorption of gas was 34 cubical inches. Barometer 29·8; thermometer 47°.

Experiment III.

Ten grains of phosphorus. By weighing the tube after the experiments, and then distilling and burning the residual phosphorus, it was found that 9·1 grains of phosphorus had been burnt, which had absorbed 35·25 cubical inches of oxygen. Barometer 29·7; thermometer 49° Fahrenheit.

I give these experiments as the most accurate I have made. The pressure and temperature vary so little, that the corrections for them are of no importance. Supposing that 100 cubical inches of oxygen (the barometer being between 29·8 and 29·6, and the thermometer between 46° and 49° Fahrenheit) weigh 33·9 grains, phosphoric acid will be composed, according to the first result, of 100 phosphorus to 135 oxygen; according to the second, of 100 to 137·2; and according to the third, of 100 to 131·3; the mean will be 100 to 134·5.

The light of the phosphorus burning in vapour in these experiments was excessively bright; yet the top of the retort never became softened; and the phosphoric acid, which increased the weight of the tube, principally combined with the glass at the aperture where it was red hot. I cannot but consider this process of burning phosphorus in the gaseous state in a great excess of oxygen, as the most accurate mode that has yet been devised of ascertaining the composition of phosphoric acid. In this instance no phosphorous acid, as I ascertained by direct trials, is formed from the vapour; and no substances are concerned except those that actually combine. M. Dulong's method of ascertaining the composition of phosphoric acid, appears to me much too complicated to afford any results approaching to accuracy. He first combines copper wire with phosphorus, by passing phosphorus over it by means of a stream of hydrogen gas; he then dissolves the phosphuret of copper in nitric acid, and determines the quantity of phosphoric acid formed by precipitation: in all of which processes sources of error may exist.

M. Berzelius's methods of ascertaining the composition of phosphoric acid, that of reviving gold from its oxide by means of phosphorus, and that of determining the quantities of phosphate and muriate of silver formed from perphosphorane, or the perchloride of phosphorus, appear to me still more exceptionable; yet his results on the quantity of oxygen approach nearer to mine than those of M. Dulong.

The facts which I have endeavoured to establish respecting chlorine, in a paper published in the Philosophical Transactions for 1810, show that the proportional or equivalent volume in which chlorine combines, is to that in which oxygen combines, as 2 to 1; and it follows, that ten grains of phosphorus in forming the white sublimate, or perchloride, ought to combine with between 76 and 80 cubical inches of chlorine.

In experiments that I formerly made on this subject, by admitting chlorine to phosphorus in exhausted vessels, and ascertaining the absorption by introducing solution of chlorine, I overrated the absorption. I did not at that time know, what I have since ascertained, that a solution of chlorine in water, *apparently* saturated with chlorine, by agitation with it in long narrow vessels, will still take up more, by exposure to a great surface of chlorine in larger vessels. Under all circumstances, it is difficult to gain very precise results in experiments on the action of phosphorus or chlorine. Mercury acts so rapidly upon chlorine, that it cannot be employed in experiments in which the absorption is to be determined. When common water is used, some of the gas is absorbed by the water, and the sublimate being a very volatile substance, its vapour always increases its volume of the residual gas. Some aqueous vapour likewise, in experiments over water, enters with the gas, which forms a volatile hydrate, the effect of which is likewise to diminish the apparent absorption of chlorine.

I have always found the absorption greatest, when I have operated in small retorts, connected by small stop-cocks, with the vessel containing the chlorine, over water. Making the proper corrections for the absorption by the water, the apparent absorption has been from 35 to 38 cubical inches for every 5 grains of phosphorus.

M. Dulong's two methods of ascertaining the quantity of chlorine in the sublimate, appear to me at least as objectionable as his process for determining the composition of phosphoric acid, and liable to great errors: the first from the uncertainty of the absolute quantity of chlorine admitted; and the second from the loss arising from the vapour of the sublimate, which must be carried off by the current of chlorine. How great a deficiency may originate from the last circumstance, is shown by the following experiments: 5 grains of phosphorus were converted into sublimate by chlorine in great excess, the remaining chlorine was displaced by passing common air through the vessel for some time, till not the slightest smell of chlorine could be perceived; the retort was then weighed, and a current of air passed through it. Though this current could hardly have replaced the air contained in the retort, yet the loss of weight was 1·7 grains, and copious vapours were produced in the atmosphere. In a second trial of the same kind, there was a greater loss of weight, and by barely exhausting the retort, and then again admitting air, there was a loss of $\frac{7}{10}$ of a grain.

When chlorine is made to act upon phosphorus over mercury not carefully dried, some muriatic acid gas is always formed; but when the mercury has been recently boiled, no effect of this kind is produced, and the vapour in the gas forms a minute quantity of a liquid hydrate of the perchloride, which by more water, is converted into muriatic and phosphoric acids, as I proved by some very delicate experiments; so that there is certainly no hydrogen denoted in phosphorus by the action of chlorine, and in their mutual action a mere binary compound of the two substances is formed.

After reflecting much upon the methods of combining chlorine and phosphorus, so as to gain correct results, it occurred to me, that in operating over water, and introducing a perfectly saturated solution of chlorine to absorb the vapour of the sublimate, and of its hydrate formed from the water in the chlorine, I should gain a result nearly correct. I made an experiment in this way on 4 grains of phosphorus, in a retort containing 13 cubical inches. I ascertained the absorption, introduced into the retort a tube, containing about half a cubical inch of saturated solution of chlorine, and suffered the fluid slowly to act upon the sublimate, cooling the retort by immersion in water; I then ascertained the degree of the second absorption, which was nearly a cubical inch and a half. I likewise ascertained that water had its powers of dissolving chlorine diminished, and not increased, by uniting with phosphoric and muriatic acids, so that the apparent absorption must have been less than the real one. Adding the second absorption to the first, and making the proper corrections, the quantity of chlorine uniting to 4 grains of phosphorus was 31·9 cubical inches, barometer being 30·1 inches, and thermometer 46° Fahrenheit.

Rather a larger proportion would be given, if the correction for the presence of vapour had been made for some of the other experiments; and the result agrees exactly with the mean deduced from the absorption of oxygen in the formation of phosphoric acid; for, assuming that 100 cubical inches of chlorine weigh 76·5 grains, then the sublimate will consist of 1 of phosphorus to nearly 6 of chlorine; and taking the composition of phosphoric acid from this datum, it would consist of 100 phosphorus, and 135 of oxygen.

To ascertain the composition of phosphorous acid, I used a new method, that of converting the perchloride of phosphorus, or perphosphorane by phosphorus, into the chloride which affords phosphorous acid by the action of water. This is easily done by heating them together in a close retort; and it enables us to determine with certainty which opinion is correct,—*that* assuming the oxygen in phosphorous acid to be 3, or *that* which supposes it to be 2·5, the oxygen in phosphoric acid being 5.

5 grains of phosphorus were converted into perchloride in a small retort of the capacity of 6 cubical inches: it was necessary to exhaust this retort twice to remove the residual air mixed with the chlorine, and some perchloride must have been lost during this process. A small quantity of chlorine, which could have been little more than sufficient to compensate for the loss, remained in the retort. 5 grains of phosphorus were introduced, and the retort suffered to remain filled, principally with common air; heat was very slowly

applied; all the phosphorus, except an atom not so big as the head of a small pin, disappeared, and a little of the sublimate still remained, when the retort burst from the expansion of the vapour of the new chloride formed; but the chloride found on the fragments was pure, and held no phosphorus in solution.

A second experiment was made in a retort of the capacity of 11 cubical inches. 5 grains of phosphorus were converted into perchloride: the retort was twice completely exhausted, by which at least a grain and a half or two grains of perchloride must have been lost. 5 grains of phosphorus were introduced; a little of the sublimate was lost by falling into the stop-cock of the retort; yet the conversion of the phosphorus by heat into the liquor was almost complete; there remained only a minute fragment. In this experiment, however, the liquor held phosphorus in solution. When this phosphorus was precipitated by water, and obtained with the fragment by sublimation in a small glass tube, it did not equal $\frac{7}{10}$ of a grain, and was not more than could be expected from the loss of the sublimate.

The two experiments prove distinctly that the oxygen in phosphorous acid is half that in phosphoric acid; for if the proportion had been that which M. Dulong and M. Berzelius indicate, 1·67 grains of phosphorus, at least, ought to have remained after the action of the sublimate.

A collateral experiment was made. 32·7 grains of the fluid chloride, made by passing phosphorus through corrosive sublimate in great excess, were acted on by water, and precipitated by nitrate of silver; the precipitate was immediately separated from the fluid, after it had been greatly diluted with distilled water. Distilled water was then repeatedly passed through it, and it was dried and fused, when it weighed 98·4 grains; which, allowing 24·5 per cent. of chlorine in horn-silver, would give the composition of the fluid chloride as 24·108 of chlorine, and 8·592 of phosphorus.

The comparative quantity of precipitate in this experiment was so much less than I had found in a former experiment, that notwithstanding the care with which the process had been conducted, I resolved to make some more experiments of the same kind. In the first, in which the decomposition by water was made in a small bottle, from which no vapour could escape, and in which I superintended the weighing and drying of the horn silver formed, with the greatest care, 18·4 of the liquid chloride afforded only 54·5 of chloride of silver, which agrees as nearly as could be expected with the former experiments. In two other experiments, made with equal care, and in which the liquid was poured into a solution of nitrate of silver, 6 grains gave 17·1 of horn-silver, and 29·4 gave 89·9 of fused horn-silver.

In examining minutely the circumstances of the action of the liquid chloride, or solutions containing phosphorous and muriatic acids, or nitrate of silver, I found no difficulty in explaining the cause of the error in the former experiments. Phosphorous acid acts upon nitrate of silver, and more rapidly in proportion to its concentration, and gradually produces a copious precipitate from it; so that if there be an excess of nitrate of silver, and the precipitate be not immediately separated from the solution, there is always a considerable increase of weight. M. Dulong and M. Berzelius, whose experiments agree with my former ones, *may* have been misled by a precipitation from the nitrate of silver by phosphorous acid, as I am sure *I* was. M.

Berzelius does not state how he prepared the liquid chloride of phosphorus; but M. Dulong, who objects to my process by corrosive sublimate, and employs, instead of it, the action of chlorine on phosphorus in forming his fluid, must have been exposed to other sources of error. He speaks of acting on dry phosphorus by dry chlorine; but it must be always extremely difficult to free a gas, that cannot be kept over mercury, of all its vapour; and as perchloride always forms during the action of phosphorus on chlorine, a part of which produces a fluid, and easily volatile hydrate with water, and soluble in all proportions in the liquid chloride, this process must be very liable to error. I have never been able to form the perchloride, even from chlorine slowly passed through muriate of lime, without producing a small quantity of liquid hydrate of perchloride, which, when the solid perchloride was converted into liquid by more phosphorus, rose in vapour with it, and which containing nearly a double quantity of chlorine (for the water forms a very small part of it), occasions the precipitation of a much larger quantity of horn-silver than the pure chloride formed from corrosive sublimate.

These various experiments on the combination of phosphorus with oxygen and chlorine, sufficiently agree with each other to afford the means of determining the proportion in which phosphorus combines with other bodies, or its equivalent number considered as an element.

If the absorption of oxygen be considered as offering the data, and phosphoric acid be supposed to consist of two proportions of oxygen, and one of phosphorus, the number representing the proportion in which phosphorus combines, will be 22·3. If the phosphoric acid be considered as consisting of 4 proportions of oxygen, the proportional number or equivalent of phosphorus will be 44·6.

If the absorption of chlorine in forming phosphorane be made the datum, the number will be the same, 22·2, or the double 44·4. If the quantity of horn-silver formed from the liquid chloride, taking the mean of all the experiments, be assumed as the datum, the number would be 23·5, or the double 47: the mean of all these proportions is 22·6, or the double 45·2; or, taking away decimals, 45.

In referring to the analyses which have been made of the different combinations of phosphoric acid, for the purpose of ascertaining if they correspond with this number, I found the data so uncertain and so discordant, that it was impossible to form any conclusions from them. The phosphate of soda, as is well known, has alkaline properties; yet, according to M. Berzelius, it contains but 17·67 of soda to 20·33 of acid; whereas it ought to contain, according to the proportion indicated by my experiments (if neutral), nearly an equal weight of soda. M. Berzelius mentions several combinations of baryta and lime with phosphoric acid, of which only two approach to a correspondence with the number I have given for phosphorus; that containing 45·5 of acid to 46·7 of lime; and that containing 39·1 of acid to 60·8 of barytes. New researches are required to explain the anomalies presented by the phosphates. I shall give three experiments on the quantity of hydrate of potassa necessary for saturating given quantities of phosphoric acid made from given weights of phosphorus.

18 grains of phosphorus converted into phosphoric acid by combustion in oxygen, required for its saturation 47 grains of dry hydrate of potassa.

5·7 grains of phosphorus converted into acid, required 14·7 of hydrate of potassa.

5 grains of phosphorus converted into perchloride, demanded, to produce perfect neutralization, 68 grains of hydrate of potassa.

These three experiments agree so well with each other, and with the proportionate number gained from the absorption of chlorine and oxygen by phosphorus, that it is impossible not to put confidence in them.

If 13·1 be considered as the quantity of hydrate of potassa required to neutralize the phosphoric acid formed in the last experiment, and the 54·9 of hydrate remaining, be supposed to contain 43 grains of potassa, then the chlorine required to expel the oxygen from the potassa would be rather more than 40 cubical inches.

We owe to the ingenuity of M. Dulong the discovery of an acid, which he names the hypophosphorous acid, and which he supposes to contain half the quantity of oxygen in the phosphorous acid. I have satisfied myself as to the correctness of his views respecting the existence of this acid, and the properties of its compounds; but I cannot regard the method he has adopted for its analysis as entitled to confidence. He takes a given quantity of hypophosphite of soda, acts upon this by chlorine, converts the excess of chlorine into muriatic acid, precipitates by nitrate of silver and earthy salts, and from the comparison of all these data, in which some substances of uncertain composition may be concerned, draws his conclusions.

I have found that the neutral hypophosphite of barytes, when acted on by heat in close vessels, is converted into acid phosphate of barytes, disengaging an elastic fluid, which is almost entirely the hydrophosphoric gas, or phosphuretted hydrogen saturated with phosphorus. I say *almost entirely*, because, in the beginning of the process, a little gas spontaneously inflammable is produced, and a minute quantity of moisture appears: and when the heat is raised to redness, a very little phosphorus is produced, probably from the decomposition of a part of the phosphoric gas. Now supposing the quantity of phosphoric acid in the phosphate of baryta known, and the quantity of phosphorus in phosphuretted hydrogen known; it is very easy, from an accurate experiment on the decomposition of the hypophosphite of baryta, to learn the composition of hypophosphorous acid.

I made two experiments on this subject; in one, 50 grains of dry hypophosphite of barytes were used, and the distillation conducted in a small glass tube. About 23·25 cubical inches of gas were produced. The loss of weight of the apparatus could not be ascertained, as unluckily a little of the phosphate was lost; a small portion of phosphorus was deposited in the upper part of the tube, from the decomposition of a minute quantity of the bi-phosphuretted gas; but this could not have equalled the $\frac{4}{10}$ of a grain, as the tube only lost $\frac{4}{10}$ by being heated to whiteness.

In the second experiment, 29 grains of the hypophosphite were used, and the loss of weight only ascertained, which was 3·5 grains. To be able to form any opinion as to the composition of the hypophosphorous acid, it was necessary to ascertain the composition of the phosphate of baryta produced in these experiments; which was easily done by precipitating a given quantity of the hypophosphite of barytes by sulphate of soda in solution. 15 grains of hypophosphite of barytes, in an experiment very carefully made, afforded 11·3 of sulphate of barytes. Now, supposing this sulphate of barytes to contain 7·4 of baryta, the hypophosphite would consist of 7·4 of baryta, and 7·6 of hypophosphorous acid; an 13·1 of the acid phosphate of baryta, formed from its decomposition, would contain 5·7 phosphoric acid, and 7·4 baryta. And in the experiment in which 29 grains of hypophosphite of baryta were decomposed, supposing the whole loss of weight to be owing to the perphosphuretted hydrogen given off, and this gas to be composed of 22·5 of phosphorus to 4 of hydrogen, or of 5·29 hydrogen to 29·76 phosphorus, and the 25·5 of acid phosphate remaining composed of 14·47 baryta nearly, and 11·03 phosphoric acid, adding the 29·76 of phosphorus to the 4·72 in the phosphoric acid, and subtracting 39, the quantity of oxygen required to form water with the 5·24 of hydrogen, the hypophosphorous acid may be conceived to be composed of 7·69 phosphorus, and 2·54, which denotes rather less than half the oxygen in phosphorous acid; i. e. as 7·43 to 1·5, an approximation nearer than could have been expected.

Assuming the composition of the phosphuretted gas to be what is stated in the preceding page, which agrees very nearly with an experiment which I formerly made, the first experiment on the quantity of gas disengaged would give a proportion of oxygen rather less than that which has been just calculated upon; but it must be remembered, that a certain quantity of common phosphuretted hydrogen is produced, which, containing less hydrogen in a given volume, would sufficiently explain the difference of result.

M. Dulong has advanced an ingenious opinion, that the hydrophosphorous acid *may be considered* as a triple compound of hydrogen, oxygen, and phosphorus. There is another view which may be taken of its composition, namely, that it may be a compound of phosphoric acid and perphosphuretted hydrogen. Phosphuretted hydrogen, as may be deduced from some experiments of M. Dulong, has the properties of a very weak alkali; and when expelled from the neutral hypophosphites, they become acid. This view agrees very well with the equivalent, or proportional numbers, which represent the phosphoric acid and phosphuretted hydrogen. If it be adopted, the hypophosphites must be considered as triple compounds, analogous to the salts containing fixed alkali and earths, or ammonia and earths combined with acids.

M. Dulong imagines that the acid formed by the slow combustion of phosphorus in the air, and which I have supposed to be a mixture of phosphorous and phosphoric acids, is a peculiar acid, or chemical compound of phosphorous and phosphoric acids, which he names phosphatic acid. I cannot say that his arguments give much probability to this opinion. This substance has no crystalline form, no marked character which distinguishes it from a mere mixture of phosphorous and phosphoric acids; and as far as my experiments have gone, it is far from uniform in its composition; and phosphorous and phosphoric acids mixed together, produce a substance of exactly the same kind.

That a mixture of phosphorous and phosphoric acids should be produced by the slow combustion of phosphorus, is not surprising, when it is considered, that this phenomenon is connected with different chemical processes, viz., the action of the vapour of phosphorus upon air, the action of solid phosphorus upon the elastic atmosphere, and upon the air dissolved in the moisture attracted by the acids formed; and, unless vapour be present in the air, the process of the slow conversion of phosphorus into acids soon stops.

I have mentioned in the paper to which I have referred in the beginning of this communication, that the hydrophosphorous acid is decomposed by heat; and that phosphoric acid and perphosphuretted hydrogen are the results. In examining the nature of the phosphoric acid formed, I find that it contains water, so that it is a hydrated phosphoric acid. In carefully conducting the experiment, I find likewise, that a small proportion of water is given off with the perphosphuretted gas. I shall give the results of an experiment: 17·5 grains of hydrophosphorous acid were decomposed by heat in a small glass retort carefully weighed; 6·5 cubical inches of elastic fluid were generated, and the loss of the retort was 4 grains. Now, if it be assumed that the hydrate of phosphoric acid* remaining equalled 13·5 grains, and that it contained, according to the law of definite proportions, 1·88 of water, and that the bi-phosphuretted gas weighed 1·937, and consisted of 1·6446 phosphorus, and ·2924 hydrogen; then the oxygen in the phosphorous acid will be to the phosphorus as 44 to 66, which is as near a result as can be expected.

For 4 proportions of phosphorous acid are	300	or the double	150
and 10 of water	170	or	85
which together amount to	470	or	235
which form 3 proportions of phosphoric acid	315	or	157·5
with 3 of water to form the hydrate	51	or	25·5
	366		183·0

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* I proved it to be a hydrate by heating it with magnesia, when abundance of water was given off from it.

4 of water decomposed, of which the hydrogen is 8, to form with 45 of phosphorus phosphuretted hydrogen	53	or	26·5
3 of water given off	51	or	25·5
making	470	or	235

I have no doubt that the acid which I used formerly was drier than the acid employed in this experiment, which will account for the difference of the result. Supposing a hydrophosphorous acid could be procured, containing only the quantity of water sufficient to convert it into dry phosphoric acid, it would consist, as I have stated in my former paper on phosphorus, of 4 proportions of water, and 4 proportions of phosphorous acid.

I have adopted throughout the whole of these calculations, the supposition that the hydrogen in water is to the oxygen as 2 to 15; and consequently I have taken the number representing oxygen as 15, which is extremely convenient, as the multiples are simple, 30, 45, 60, &c. Taking the proportion of phosphoric acid in phosphate of potassa, which may be deduced from the experiments, page 329, it appears more convenient to represent the proportional number, or equivalent of phosphorus, by 45, or 45·2, than by 22·5, or 22·6, which gives facility in adopting either hypothesis of the composition of hypophosphorous acid. If it be supposed a simple compound of oxygen and phosphorus, the series of proportions in the acids will be

Hypophosphorous acid, Phosphorus...	45	oxygen	15
Phosphorous acid	45	oxygen	30
Phosphoric acid..........................	45	oxygen	60

Or hypophosphorous acid 263	Phosphoric acid 2 proportions	210
	Posphuretted hydrogen 1 proportion	53

I shall conclude this paper by a few incidental observations on the compounds of phosphorus.

M. Dulong states that no phosphorous acid is formed when phosphorus is burnt in excess of oxygen or atmospheric air; as, he says, I have asserted. I cannot find that I have any where made such an assertion; but notwithstanding what M. Dulong pretends, the assertion is true, as the following experiments will prove. Half a grain of phosphorus was set fire to in a retort containing 16 cubical inches of common air; the acid products were washed with distilled water, and passed through a filter, and evaporated. When the acid became nearly dry, small globules of phosphuretted hydrogen were disengaged from it, indicating the presence of phosphorous acid. The experiment was repeated two or three times, care being taken to separate the red powder which has been considered as an oxide of phosphorus, and always with the same result.

Whenever phosphorus is inflamed, and suffered to become extinguished in oxygen gas in excess, unless the *product* is strongly heated after the spontaneous combustion is over, an acid, of which the hydrate produces phosphuretted hydrogen by heat, is always found in the products; and this acid is probably produced by the action of the solid phosphorus on the phosphoric acid in contact with it. This fact, and the circumstance, that much phosphorous acid is produced by the combustion of phosphorus in rare air, renders it almost certain that the phosphorous acid is a direct combination of phosphorus and oxygen, and destroys an idea which might otherwise be formed from the phenomena of the decomposition of its hydrate, namely, that it is a compound of 3 proportions of phosphoric acid, and 1 of phosphuretted hydrogen.

M. Dulong and M. Berzelius speak of freeing phosphorane, or the liquid chloride of phosphorus, from phosphorus by distillation. In experiments made in the laboratory of the Royal Institution, in which it has been twice carefully distilled at a low heat, it has still contained minute quantities of phosphorus.

It has been supposed that dry phosphoric acid is fixed at a white heat; but I find that this is not the case: it rapidly rises in vapour at this temperature, and evaporates even at the point of fusion of flint glass: and the hydrate of phosphoric acid is susceptible of being volatilized at a much lower temperature.

In converting the solid sublimate composed of phosphorus and chlorine into the liquid compound, when the phosphorus is first fused in contact with the sublimate, a yellow crystalline mass is formed, which, when acted on by a higher degree of heat, affords the liquid chloride, which rises from it in vapour, and leaves phosphorus behind. It is possible that this yellow solid is a compound of phosphorus and chlorine, containing half as much chlorine as the liquid. Should this be proved to be the case by future experiments, it will give weight to the idea, that the hypophosphorous acid is a binary compound of oxygen and phosphorus

XII.

ON A NEW DETONATING COMPOUND.—IN A LETTER TO THE RIGHT HON. SIR JOSEPH BANKS, BART. K.B. P.R.S.[*]

My dear Sir,

I think it right to communicate to you, and through you to the Royal Society, such circumstances as have come to my knowledge respecting a new and a very extraordinary detonating compound. I am anxious that those circumstances should be made public as speedily as possible, because experiments upon the substance may be connected with very dangerous results; and because I have already mentioned the mode of preparing it to many of my chemical friends, to whom my experience may be useful in saving them from danger.

About the end of September, I received a letter from a philosophical gentleman at Paris on some subjects of science, which contained the following paragraph: "Vous avez sans doute appris, Monsieur, la découverte qu'on a faite à Paris il y a près d'un an, d'une combinaison de gaz azote et de chlorine, qui a l'apparence d'une huile plus pesante que l'eau, et qui détone avec toute la violence des métaux fulminans à la simple chaleur de la main, ce qui a privé d'un œil et d'un doigt l'auteur de cette découverte. Cette détonation a lieu par la simple separation des deux gaz, comme celle de la combinaison d'oxigène et de chlorine; il y a également beaucoup de lumière et de chaleur produites dans cette détonation, où un liquide se decompose en deuz gaz."

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[*] [From the Phil. Trans. for 1813. Read before the Royal Society, Nov. 5, 1812.]

The letter contained no account of the mode of preparation of this substance, nor any other details respecting it.

So curious and important a result could not fail to interest me, particularly as I have long been engaged in experiments on the action of azote and chlorine, without gaining any decided proofs of their power of combining with each other. I perused with avidity the different French chemical and physical journals, especially *Les Annales de Chimie*, and *Le Journal de Physique*, of which the complete series of last year have arrived in this country, in hopes of discovering some detail respecting the preparation of this substance, but in vain. I was unable to find any thing relative to it in these publications, or in the *Moniteur*.

It was evident from the notice, that it could not be formed in any operations in which heat is concerned; I therefore thought of attempting to combine azote and chlorine under circumstances which I had never tried before, that of presenting them to each other artificially cooled, the azote being in a nascent state. For this purpose I made a solution of ammonia, cooled it by a mixture of ice and muriate of lime, and slowly passed into it chlorine, cooled by the same means. There was immediately a violent action, accompanied by fumes of a peculiarly disagreeable smell; at the same time a yellow substance was seen to form in minute films on the surface of the liquor; but it was evanescent, and immediately resolved itself into gas. I was preparing to repeat the experiment, substituting the prussiate of ammonia and other ammoniacal compounds, in which less heat might be produced by the action of the chlorine, than in the pure solution of the gas, when my friend, Mr. J. G. Children, put me in mind of a circumstance of which he had written to me an account, in the end of July, which promised to elucidate the inquiry, viz. that Mr. James Burton, jun. in exposing chlorine to a solution of nitrate of ammonia, had observed the formation of a yellow oil, which he had not been able to collect so as to examine its properties, as it was rapidly dissipated by exposure to the atmosphere. Mr. Children had tried the same experiment with similar results.

I immediately exposed a phial, containing about six cubical inches of chlorine to a saturated solution of nitrate of ammonia, at the temperature of about 50° in common day-light. A diminution of the gas speedily took place; in a few minutes a film, which had the appearance of oil, was seen on the surface of the fluid; by shaking the phial it collected in small globules, and fell to the bottom. I took out one of the globules, and exposed it in contact with water to a gentle heat: long before the water began to boil, it exploded with a very brilliant light, but without any violence of sound.

I immedialely proposed to Mr. Children, that we should institute a series of experiments upon its preparation and its properties. We consequently commenced the operations, the results of which I shall describe. We were assisted in our labours, which were carried on in Mr. Children's laboratory at Tunbridge, by Mr. Warburton.

It was found, that the solution of oxalate of ammonia, or a very weak solution of pure ammonia, answered the purpose as well as the solution of nitrate of ammonia. It was formed most rapidly in the solution of ammonia, but it was white and clouded; and though less evanescent than in the strong solution I first used, it was far from being as permanent as in the solutions of nitrate and oxalate. The solution of prussiate of ammonia acted on by chlorine, afforded none of the peculiar oil; but produced white fumes, and became of a bright green colour. An attempt was made to procure the substance in large quantities, by passing chlorine into Wolfe's bottles, containing the different solutions, but a single trial proved the danger of this mode of operating; the compound had scarcely began to form, when, by the action of some ammoniacal vapour on chlorine, heat was produced, which occasioned a violent explosion, and the whole apparatus was destroyed.

I shall now describe the properties of the new substance. Its colour is very nearly that of olive oil, and it is almost as transparent. It congeals at a temperature of about 32°, and then has the appearance of butter. I have not been able to ascertain its specific gravity with accuracy, but it is probably about 1·4. Its smell is very nauseous, strongly resembling that of the combination of carbonic oxide and chlorine, discovered by my brother; and its effect on the eyes is peculiarly pungent and distressing. A little of it was introduced under water into the receiver of an air pump, and the receiver exhausted; it became an elastic fluid, and in its gaseous state was rapidly absorbed or decomposed by the water. When warm water was poured into a glass containing it, it expanded into a globule of elastic fluid, of an orange colour, which diminished as it passed through the water.

I attempted to collect the products of the explosion of the new substance, by applying the heat of a spirit lamp to a globule of it, confined in a curved glass tube over water: a little gas was at first extricated, but long before the water had attained the temperature of ebullition, a violent flash of light was perceived, with a sharp report; the tube and glass were broken into small fragments, and I received a severe wound in the transparent cornea of the eye, which has produced a considerable inflammation of the eye, and obliges me to make this communication by an amanuensis. This experiment proves what *extreme* caution is necessary in operating on this substance, for the quantity I used was scarcely as large as a grain of mustard seed.

A small globule of it thrown into a glass of olive oil, produced a most violent explosion; and the glass, though strong, was broken into fragments. Similar effects were produced by its action on oil of turpentine and naphtha. When it was thrown into ether there was a very slight action; gas was disengaged in small quantities, and a substance like wax was formed, which had lost the characteristic properties of the new body. On alcohol it acted slowly, lost its colour, and became a white oily substance, without explosive powers. When a particle of it was touched under water by a particle of phosphorus, a brilliant light was perceived under the water, and permanent gas was disengaged, having the characters of azote.

When quantities larger than a grain of mustard seed were used for the contact with phosphorus, the explosion was always so violent as to break the vessel in which the experiment was made. The new body, when acted upon under water by mercury, afforded a substance having the appearance of corrosive sublimate, and gas was disengaged. On tin foil and zinc it exerted no action; it had no action on sulphur, nor on resin. In their alcoholic solutions it disappeared as in pure alcohol. It detonated most violently when thrown into a solution of phosphorus in ether, or in alcohol. Phosphorus introduced into ether, into which a globule of the substance had been put immediately before, produced no effect. In muriatic acid it gave off gas rapidly, and disappeared without explosion. On dilute sulphuric acid it exerted no violent action. It immediately disappeared without explosion in Libavius's liquor, to which it imparted a yellow tinge.

It seems probable, from the general tenour of these facts, that the new substance is a compound of azote and chlorine; the same as, or analogous to, that mentioned in the letter from Paris. It is easy to explain its production in our experiments: the hydrogen of the ammonia may be conceived to combine with one portion of the chlorine to form muriatic acid, and the azote to unite with another portion of chlorine to form the new compound. The heat and light produced during its expansion into gaseous matter, supposing it to be composed of azote and chlorine, is without any parallel instance, in our present collection of chemical facts; the decomposition of euchlorine, which has been compared to it, is merely an expansion of matter already gaseous. The heat and light produced by its rarefaction, in consequence of decomposition, depend, probably, on the same cause as that which produces the flash of light in the discharge of the air-gun.

The mechanical force of this compound in detonation, seems to be superior to that of any other known, not even excepting the ammoniacal fulminating silver. The velocity of its action appears to be likewise greater.

I am, my dear Sir,
With great respect, very sincerely your's,
H. DAVY.

XIII.

SOME FURTHER OBSERVATIONS ON A NEW DETONATING SUBSTANCE.—IN A LETTER TO THE RIGHT HON. SIR JOSEPH BANKS, BART. K.B. P.R.S.*

MY DEAR SIR,

Berkeley-square, June 20, 1813.

I HAVE already described, in a letter which you were so good as to communicate to the Royal Society, a few facts respecting a new detonating compound. I shall now do myself the honour of mentioning to you some other particulars on the subject.

I received, in April, a duplicate of the letter in which the discovery was announced, containing an Appendix, in which the method of preparing it was described. M. Ampere, my correspondent, states that the author obtained it by passing a mixture of azote and chlorine through aqueous solutions of sulphate, or muriate of ammonia. It is obvious, from this statement, that the substance discovered in France, is the same as that which occasioned my accident. The azote cannot be necessary; for the result is obtained by the exposure of pure chlorine to any common ammoniacal salt.

[* From the Philosophical Transactions for 1813. Read before the Royal Society, July 1st, 1813.]

Since I recovered the use of my eyes, I have made many experiments on this compound; it is probable that most of them have been made before in France; but as no accounts of the investigations of M. Dulong on the substance have appeared in any of the foreign journals which have reached this country, and as some difference of opinion and doubts exist respecting its composition, I conceive a few details on its properties and nature will not be entirely devoid of interest.

I have been able to determine its specific gravity, I hope, with tolerable precision, by comparing its weight at 61° Fahrenheit, with that of an equal volume of water: 8·6 grains of the compound, carefully freed from the saline solution in which it was produced, filled a space equal to that filled by 5·2 grains of water, consequently its specific gravity is 1·653.

When the compound is cooled artificially, either in water or in solution of nitrate of ammonia, the fluid surrounding it congeals at a temperature a little below 40° Fahrenheit, which seems to be owing to its becoming a solution of chlorine; for, as I have stated in a paper published in the Philosophical Transactions, the saturated solution of chlorine in water freezes very readily. The congelation of the fluid, in contact with the new compound, led me, when I first operated on it in very small quantities, to suppose it readily rendered solid by cooling; but I find in experimenting upon it, out of the contact of water, that it is not frozen by exposure to a mixture of ice and muriate of lime.

The compound gradually disappears in water, producing azote, and the water becomes acid, and has the taste and smell of a weak solution of nitro-muriatic acid.

The compound, when introduced into concentrated solution of muriatic acid, quickly resolves itself into gas, producing much more than its own weight of elastic fluid, which proves to be pure chlorine, and the solution evaporated affords muriate of ammonia.

In concentrated nitric acid it afforded azote.

In diluted sulphuric acid it yielded a mixture of azote and oxygen.

It detonated in strong solutions of ammonia. In weak solutions it produced azote.

It united to or dissolved in sulphurane, phosphorane, and alcohol of sulphur, without any violence of action, and dissolved in moderately strong solution of fluoric acid, giving it the power of acting upon silver.

When it was exposed to pure mercury, out of the contact of water, a white powder and azote were the results.

The first attempt that I made to determine the composition of the detonating substance, after my accident, was by raising it in vapour in exhausted vessels, and then decomposing it by heat; but in experiments of this kind, even though the whole of the substance was expanded into elastic matter, yet the vessel was often broken by the explosion, and in several instances violent detonations occurred during the process of exhaustion, probably from the contact of the vapour of the substance with the oil used in the pump.

In the only instance in which I was able to examine the products of the explosion of the substance in an exhausted vessel, no muriatic acid or water was formed, and chlorine and azote were produced; but it was impossible to form any correct opinion concerning the proportions of the gaseous matter evolved, as an unknown quantity of common air must have remained mixed with the vapour in the vessel.

The action of mercury on the compound appeared to offer a more correct and less dangerous mode of attempting its analysis; but on introducing two grains under a glass tube filled with mercury and inverted, a violent detonation occurred, by which I was slightly wounded in the head and hands, and should have been severely wounded, had not my eyes and face been defended by a plate of glass attached to a proper cap, a precaution very necessary in all investigations of this body.

In using smaller quantities and recently distilled mercury, I obtained the results of the experiments, without any violence of action; and though it is probable that some accidental circumstance might have occasioned the explosion of the two grains, yet I thought it prudent, in my subsequent experiments, to employ quantities which, in case of detonation, would be insufficient to do any serious mischief.

In the most accurate experiment that I made, $\frac{7}{10}$ths of a grain of the compound produced, by its action upon mercury, 49 grain measures of azote. I collected the white powder which had been formed in this and other operations of the same kind, and exposed it to heat. It sublimed unaltered, without giving off any elastic or fluid matter, which there is the greatest reason to believe would not have happened, if the compound had contained hydrogen, or oxygen, or both. The sublimed substance had the properties of a mixture of corrosive sublimate and calomel.

If the results of this experiment be calculated upon, it must be concluded that the compound consists of 57 of azote to 643 of chlorine in weight, or 19 to 81 in volume; but this quantity of azote is probably less than the true proportion, as there must have been some loss from evaporation, during the time the compound was transferred, and it is possible that a minute quantity of it may have adhered to mercury not immediately within the tube.

The decomposition in this process is very simple, and must be supposed to depend merely upon the attraction of the mercury for chlorine, in consequence of which the azote is set free; and if the result does not strictly demonstrate the proportions of chlorine and azote in the compound, yet it seems at least to shew, that these are its only constituents.

As muriate of ammonia and chlorine are the only products resulting from its action upon solution of muriatic acid, it seems reasonable to infer, that this action depends on a decomposition of part of the muriatic acid, by the attraction of the azote of the new compound for hydrogen to form ammonia, which, at the moment of its production, combines with another portion of the acid, the chlorine of both compounds being set free.

On this view, the quantity of chlorine formed from a certain quantity of the compound being known, it becomes easy to determine the composition of the compound; for, ammonia being formed of three volumes of hydrogen and one of azote, and muriatic acid of one volume of hydrogen and one of chlorine, it is evident, that for every three volumes of chlorine evolved by the decomposition of muriatic acid, one volume of azote must be detached from the compound; and the weight of chlorine in the compound must be less than the weight of the whole quantity of chlorine produced,—by a portion which is to the azote in the compound as 295 to 2295, if the relative specific gravities of the two gases be considered as 2·627 and 1.

Two grains of the compound, when exposed at the temperature of 62° Fahrenheit, and under a pressure of the atmosphere equal to that of 30·1 inches of mercury to strong solution of muriatic acid in a proper apparatus, afforded 3·91 cubic inches of chlorine.

In another experiment, one grain of the compound afforded 1·625 cubic inches of chlorine.

In a third experiment, one grain produced only 1·52 cubic inches.

In the two last experiments the compound was acted upon much more slowly, and the gas generated exposed to a much larger surface of solution of muriatic acid, and the appearance of a smaller relative proportion of chlorine must be ascribed to the absorption of a larger proportion of that gas by the liquid acid; and I found by exposing concentrated solution of muriatic acid to chlorine, that it soon absorbed nearly its volume of that gas.

I attempted to remove the source of error in the experiment, by using liquid muriatic acid holding chlorine in solution; but in this case the quickness of the action of the compound on the acid was greatly diminished, and it not being easy to obtain the point of absolute saturation of the acid with chlorine, some of the gas was absorbed in the nascent state during its slow production; and in most of my experiments made in this manner, I obtained less chlorine from a given weight of the compound, than in operating on pure solution of muriatic acid.

Liquid muriatic acid, whether concentrated or diluted in its pure state, does not affect the colour of the sulphuric solution of indigo; but it is immediately destroyed by solutions containing chlorine dissolved in them. The quantity of solution of indigo, which is deprived of colour by a given quantity of solution of chlorine, is directly as the proportion of chlorine it contains; and I found that the same quantity of chlorine, whether dissolved in a large or a small quantity of solution of muriatic acid, destroyed the colour of the same quantity of the blue liquor.

On this circumstance it was easy to found a method of determining the precise quantity of chlorine produced in solution of muriatic acid, from a given quantity of the compound; namely, by comparing the power of a given quantity of muriatic acid, containing a known quantity of chlorine, to destroy the colour of solutions of indigo, with that of the muriatic acid, in which the compound had produced chlorine.

Two experiments were made. In the first, a grain of the compound was exposed on a large surface beneath a tube inverted in about six cubic inches of solution of muriatic acid, and the chlorine absorbed by agitation as it was formed. The acid so treated destroyed the colour of seven cubic inches of a diluted sulphuric solution of indigo; and it was found, by several comparative trials, that exactly the same effect was produced in another equal portion of the same solution of indigo, by 2·2 cubic inches of chlorine dissolved in the same quantity of muriatic acid.

In the second experiment, 1·3 cubic inches of chlorine were evolved in the gaseous form, the thermometer being at 58°, and barometer at 30·33, and suffered to pass into the atmosphere; and by the test of the solution of indigo, it was found that ·75 of a cubic inch remained dissolved in the acid.

Now, if the mean of these two experiments be taken, it appears that 1·61 grains of chlorine are produced in solution of muriatic acid by the action of a grain of the compound; and calculating on the data just now referred to, the compound must consist of 91 of chlorine and 9 of azote in weight, which in volume will be nearly 119 to 30; and this estimation differs as little as might be expected from that gained by the action of mercury upon the compound.

It may fairly be concluded, that M. Gay Lussac's principle of the combination of gaseous bodies, in definite volumes, strictly applies to this compound, and that it really consists of four volumes of chlorine to one of azote; and the volumes likewise exactly coincide with the laws of definite proportions; and the detonating compound may be regarded as composed of one proportion of azote 26, and four proportions of chlorine 268.

I attempted a comparative experiment on the proportions in the compound, by estimating the quantity of azote produced in the decomposition of ammonia by it; but I found that this process was of no value for the purpose of analysis, for water appeared to be decomposed at the same time with the ammonia, and nitric acid formed; and, in consequence, the quantity of azote evolved was much less than it would have been, supposing the ammonia decomposed by the mere attraction of chlorine for hydrogen.

The results of the analysis of the new compound are interesting for several reasons.

They show, what seemed probable from other facts, that there is no strict law of analogy, which regulates the combinations of the same substance with different substances. As three of hydrogen combine with one of azote, and one of hydrogen with one of chlorine, I thought it probable that the new compound would contain three of chlorine to one of azote, which is not the case.

This compound is the first instance known of one proportion of a substance uniting to four proportions of another substance, without some intermediate compound of one and one, one and two, and one and three; and the fact should render us cautious in adopting hypothetical views of the composition of bodies from the relations of the quantities in which they combine. Those who argue that there must be one proportion of oxygen in azote, because there ought to be six proportions in nitric acid, instead of five, which are produced from it by analysis, might with full as much propriety contend, that there must be azote in chlorine in some simple multiple of that existing in the compound.

It may be useful to show, that many hypotheses may be framed upon the same principles; and which, consequently, must be equally uncertain. Views of this nature may be important in directing the practical chemist in his researches; but the philosopher should carefully avoid the development of them with confidence, and the confounding them with practical results.

The compound of chlorine and azote agrees with the compounds of the same substance with sulphur, phosphorus, and the metals, in being a non-conductor of electricity; and these compounds are likewise decomposable by heat, though they require that of Voltaic electricity.

Sulphur combines only in one proportion with chlorine; and hence the action of *sulphurane*, or Dr. Thomson's muriatic liquor upon water, like that of the new compound, is not a simple phenomenon of double decomposition.

It seems proper to designate this new body by some name: *azotane** is the term that would be applied to it, according to my ideas of its analogy to the other bodies which contain chlorine; but I am not desirous, in the

* [It is now commonly designated chloride of azote.]

present imperfect and fluctuating state of chemical nomenclature, to press the adoption of any new word, particularly as applied to a substance not discovered by myself.

I am, my dear Sir,
Very sincerely yours,
HUMPHRY DAVY.

XIV.

SOME EXPERIMENTS AND OBSERVATIONS ON THE SUBSTANCES PRODUCED IN DIFFERENT CHEMICAL PROCESSES ON FLUOR SPAR.*

IN the Bakerian Lecture, for 1808, I have given an account of an experiment on the combustion of potassium in silicated fluoric acid gas, in which the gas was absorbed, and a fawn-coloured substance formed, which effervesced with water, and left, after its action on that fluid, a residuum which burnt when heated in oxygen, reproducing silicated fluoric acid gas; and I concluded from the phenomena, that the acid gas was decomposed in the process, that oxygen was probably separated from it by the potassium, and that the combustible substance was a compound of the siliceous and fluoric bases.

The experiment of burning potassium in silicated fluoric acid gas was made likewise by MM. Gay Lussac and Thenard, before I published any account of my researches on this phenomenon. It was indeed one of the most obvious applications of potassium, and it occurred to many others, as well as to myself, that it might be made, immediately after I discovered that metal.

MM. Gay Lussac and Thenard drew the same conclusions as I did, namely, that the acid gas was probably decomposed during the action of potassium on silicated fluoric acid; but their general views differed from mine in this respect, as they supposed, that no part of the inflammable matter was derived from silica, and they likewise reasoned on the phenomena with more caution.

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* [From the Philosophical Transactions for 1813. Read before the Royal Society, July 8, 1813.]

At the time that my conclusions were drawn, I was ignorant of the true nature of the muriatic acid. After I had tried in vain to decompose oxymuriatic gas, and after I had found that the compounds of this substance with phosphorus, sulphur, and the metals combined with ammonia without any decomposition, and produced compounds in which no oxygen could be discovered; I was forcibly struck by the analogy between the oxymuriatic and the fluoric compounds, and led to doubt of the justness of my ideas respecting the nature of fluoric acid.

I tried an experiment on the comparative quantities of fluate of lime, formed from equal volumes of silicated fluoric acid gas, one of which had been acted upon by potassium, and then exposed to solution of ammonia, the other of which had been absorbed by solution of ammonia; and I found the proportion of calcareous fluate nearly one-third larger in the latter case. This result at first seemed favourable to my early ideas, that the acid contained a peculiar inflammable basis, which was separated by the potassium, and existed in the combustible substance insoluble in water; but it could not be considered as decisive on the question, for, it occurred to me as possible, that this substance might be silicum, or the basis of silica united to a much smaller proportion of the fluoric principle, than that existing in silicated fluoric acid.

During the period that I was engaged in these investigations, I received two letters from M. Ampere, of Paris, containing many ingenious and original arguments in favour of the analogy between the muriatic and fluoric compounds. M. Ampere communicated his views to me in the most liberal manner; they were formed in consequence of my ideas on chlorine, and supported by reasonings drawn from the experiments of MM. Gay Lussac and Thenard.

Before I enter upon the detail of the investigations which promise to elucidate the nature of the fluoric compounnds, it will be right to describe those substances produced from fluor spar, which have been the principal objects of my experiments, and to mention the different hypothetical views that may be formed respecting them.

The first of these substances is the silicated fluoric acid gas, which was discovered by Scheele, and examined in its pure state by Priestley. It is formed by heating a mixture of fluor spar, powdered glass, and sulphuric acid. It is a very heavy elastic fluid, its specific gravity being nearly forty-eight times as great as that of hydrogen. It produces, according to my brother, Mr. John Davy, a quantity of silica equal to $\frac{152}{1000}$ of its own weight by its action upon water, and a quantity equal to $\frac{614}{1000}$ of its weight by its action upon solution of ammonia. It condenses twice its own volume of ammonia, and forms a solid salt, volatile when free from water without decomposition.*

Liquid fluoric acid, the second of these substances, was discovered by Scheele, but first obtained in its pure form by MM. Gay Lussac and Thenard. It is procured by heating concentrated sulphuric acid and pure fluor spar, in retorts of silver or lead, and receiving the product in receivers of the same metals artificially cooled. It is a very active substance, and must be examined with great caution.* According to my experiments, its specific gravity is 1.0609.† It produces a high degree of heat when mixed with water, and such is its degree of attraction for water, that it becomes denser by combining with that fluid. By adding water, in very small quantities at a time, to pure liquid fluoric acid, I found that its specific gravity gradually increased till it became 1·25: it is, I believe, the only known body possessed of this property.

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* [I have found that this gas combines directly with lime in the same manner as with ammonia (one proportion of the silicated fluoric acid with one of the oxide of calcium), and that the union is attended with ignition; and farther, that it likewise combines directly with magnesia, alumine, and all the oxides of the common metals, of which trial was made. Vide Edinburgh New Philosophical Journal, vol. xvii.]

The third substance is fluo-boric acid gas, which was discovered by MM. Gay Lussac and Thenard. It is produced by intensely heating, in an iron tube, a mixture of dry boracic acid and fluor spar, or by gently heating in a glass retort a similar mixture with sulphuric acid. Its specific gravity is rather more than thirty-two times as great as that of hydrogen. It forms a solid salt, volatile without decomposition, by condensing its own volume of ammonia.‡ The ammoniacal salt dissolved in water and distilled, affords boracic acid.

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* [The manner in which it excites inflammation and ulceration is very remarkable, and apparently in a much higher degree than its immediate disorganizing agency, as if it acted on living textures specifically as a poison.]

† Unless it is distilled through tubes and into vessels of pure silver, its specific gravity is greater; it readily dissolves tin, and slowly dissolves lead, and after being long kept in vessels of pure silver, it is found to have taken up a small portion even of that metal.

‡ [I have found that it combines with ammonia in two other proportions: that one volume of it is capable of condensing one, two or three volumes of the volatile alkali, and that the two latter compounds are liquid. Vide Phil. Trans. for 1812.]

The most important phenomena of chemical change, in which these bodies operate, that may be supposed to illustrate their nature, is their agency upon potassium and other metals. The action of potassium upon silicated fluoric gas has been already referred to. MM. Gay Lussac and Thenard, by heating potassium and sodium in fluo-boric acid gas, obtained fluate of potassa or soda, and the basis of the boracic acid; and by exposing potassium to liquid fluoric acid, their results were hydrogen and acid fluate of potassa.

Three hypotheses may, according to sound analogies, be formed on the nature of the fluoric combinations. In the first, which is that generally adopted, the silicated fluoric acid gas is supposed to be a compound of silica and a peculiar acid, itself consisting of inflammable matter and oxygen; fluo-boric acid gas, a compound of boracic acid and the same acid; and pure liquid fluoric acid as water combined with the acid.

In the second hypothesis, that which I have alluded to in the beginning of this paper, and that adopted by M. Ampere, the silicated fluoric acid is conceived to consist of a peculiar undecompounded principle, analogous to chlorine and oxygen, united to the basis of silica, or *silicum;* the fluo-boric acid of the same principle united to boron; and the pure liquid fluoric acid as this principle united to hydrogen.

In the third hypothesis, which probably would have been formed by the disciples of the phlogistic school of chemistry, had they been acquainted with the facts, the liquid fluoric acid is considered as an undecompounded body; and the metals and inflammable bodies as compounds of certain unknown bases with hydrogen: silicated fluoric acid gas, on this idea, must be regarded as a compound of the fluoric acid with the basis of silicum, and fluo-boric acid gas as a compound of fluoric acid and the basis of boron.

Whoever will consider, with attention, the different facts that have been brought forward by Scheele, Gay Lussac and Thenard, John Davy, and myself, will find that they will admit of explanation on either of these hypotheses; and, as in all the cases yet brought forward, of the most simple chemical action of other bodies on the fluoric substances, more than one new form of matter is produced, no explanation of the phenomena can at present be given without involving suppositions.

It is not easy to devise simple experiments to ascertain which of these hypotheses is true, yet, in admitting strict analogical reasoning, it is easy to shew which is most conformable to the general series of chemical facts.

Those acids which are known by direct experiments of decomposition by heat, to consist of oxygen, bases, and water, such as the strongest sulphuric and nitric acids and hydro-phosphorous acid, when they are acted on by ammonia, afford moisture: this is easily proved, by causing them to absorb ammoniacal gas in glass retorts, and gently heating the mixture, when water immediately appears. On this view, it occurred to me, if the liquid fluoric acid was a compound of water, and inflammable basis, and oxygen, that water ought to be produced when it was made to combine with ammonia. It was not possible to make the experiment in glass vessels, as the acid acts with great violence on glass, producing silicated fluoric acid gas. I had recourse, therefore, to an apparatus made of platina. A small tray of platina was filled with pure liquid fluoric acid, and introduced into a tube of platina connected by proper stop-cocks with a mercurial gasometer, filled with ammonia; the end of the platina tube was closed by a brass stopper, and a communication made between the ammonia and the fluoric acid; the ammonia was gradually absorbed, producing heat; and white fumes sometimes rose into the gas-holder, so that it was necessary from time to time to cut off the communication; ammoniacal gas was supplied till no more absorption took place. When the tube was quite cool, the stopper was removed, and the result examined; the interior contained a white crystalline mass, but there was no appearance of fluid.* A polished brass tube, cooled by means of ice, was held over the aperture of the platina tube, and it was gently heated till the salt began to sublime, but no moisture was found condensed in the cold tube of brass.

This experiment is unfavourable to the idea, that the liquid fluoric acid contains water; and the following result is likewise unfavourable to the idea that it consists of an inflammable basis united to oxygen. Solid and perfectly dry fluate of ammonia was introduced into a tray of platina, with about an equal quantity of potassium, and the tray was heated in a small tube of glass connected with a mercurial apparatus. A violent action took place, gas was disengaged with great violence, which remained for some time clouded; the application of heat was continued till the tube was red: it was then suffered to cool, and the results examined. Much white matter, which proved to be fluate of potassa, had been carried by the violence of the action out of the tray of platina into the glass tube; and a little potassium had sublimed in the tube. The tray contained a considerable portion of potassium, and a saline matter, which had all the characters of fluate of potassa. The gas disengaged, consisted of ammonia and hydrogen, to each other in volume nearly as two to one; but the experiment cannot be considered as decisive on this point, as no particular precautions had been taken to dry the mercury.

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* It is necessary that pure liquid fluoric acid, *i. e.* that which has the lowest specific gravity, be used for this experiment. The first time that I made it, I obtained moisture, owing to my having formed the hydrofluoric acid by means of sulphuric acid that had not been previously boiled, and which must have contained more than one proportion of water.

Now, if there had existed oxygen combined with an inflammable basis in the fluate of ammonia, it might have been expected to have been separated, or at least to have formed a new combination during the action of potassium upon the fluate of ammonia, which is the case with such ammoniacal salts as contain acids in which oxygen is an element. Thus nitrate of ammonia acted on by potassium, as I have found, affords azote and ammonia; and sulphur is partly disengaged, and partly newly combined during the agency of potassium in excess upon sulphate of ammonia.

The action of potassium upon fluate of ammonia is precisely similar to its action upon muriate of ammonia, in which as I have found, by numerous experiments, ammonia and hydrogen to each other in volume as two to one are disengaged, and muriate of potassa (*potassane*) formed.

All the hydrates, that is, all the substances which contain definite proportions of water, united to acids, alkalies, or oxides, which are fluid, or capable of being rendered fluid by heat, when exposed to the chemical agency of Voltaic electricity, undergo decomposition, and their inflammable principles, either pure or combined with a smaller proportion of oxygen, are disengaged at the negative surface in the circuit, and their oxygen at the positive surface. Thus sulphuric acid affords sulphur and hydrogen at the negative surface, and the hydrophosphorous acid, phosphuretted hydrogen and phosphorus, and nitric acid nitrous gas; and all these bodies yield oxygen at the positive surface.

I undertook the experiment of electrizing pure liquid fluoric acid, with considerable interest, as it seemed to offer the most probable method of ascertaining its real nature; but considerable difficulties occurred in executing the process. The liquid fluoric acid immediately destroys glass, and all animal and vegetable substances; it acts on all bodies containing metallic oxides; and I know of no substances which are not rapidly dissolved or decomposed by it, except metals, charcoal, phosphorus, sulphur, and certain combinations of chlorine.

I attempted to make tubes of sulphur, of muriates of lead and of copper containing metallic wires, by which it might be electrized, but without success. I succeeded, however, in boring a piece of horn-silver in such a manner, that I was able to cement a platina wire into it, by means of a spirit lamp, and by inverting this in a tray of platina filled with liquid fluoric acid, I contrived to submit the fluid to the agency of electricity in such a manner, that in successive experiments it was possible to collect any elastic fluid that might be produced. Operating in this way, with a very weak Voltaic power, and keeping the apparatus cool by a freezing mixture, I ascertained that the platina wire at the positive pole rapidly corroded, and became covered with a chocolate powder; gaseous matter separated at the negative pole, which I could never obtain in sufficient quantities to analyze with accuracy; but it inflamed like hydrogen.

No other inflammable matter was produced when the acid was pure.

In a case in which the acid had been condensed in a tube of lead, joined by a solder containing tin, a large quantity of powder separated at the negative surface of a dark colour, and which appeared to be tin mixed with a subfluate; the powder burnt when heated in the air, and gave fluoric fumes when treated by potassa and sulphuric acid.

I attempted to electrize the liquid fluoric acid, by making plumbago the positive surface; but the plumbago was quickly destroyed, a subfluate of iron was deposited on the negative surface, and the liquid became turbid and black. When a point of charcoal attached to a wire of platina was made positive, the effects were similar to those produced by a platina wire alone, for the acid speedily penetrated through the pores of charcoal, and the platina, in consequence, became a point of contact with the fluid.

I applied the power of the great Voltaic batteries of the Royal Institution to the liquid fluoric acid, so as to take sparks in it. In this case, gas appeared to be produced from both the negative and the positive surfaces; but it was probably only the undecompounded acid rendered gaseous, which was evolved at the positive surface, for during the operation the fluid became very hot, and speedily diminished. The manner in which the surrounding atmosphere became filled with the fumes of the fluoric acid, rendered it, indeed, very difficult to examine the results of any of these experiments; the dangerous action of these fumes have been described by MM. Gay Lussac and Thenard, and I suffered considerable inconvenience from their effects during this investigation. By mere exposure to them in their un-condensed state, my fingers became sore beneath the nails, and they produced a most painful sensation, which lasted for some hours, when they came in contact with the eyes.

The phenomena of the Voltaic electrization of fluoric acid, present no evidences in favour of its containing a peculiar combustible substance and oxygen; and the most simple mode of explaining them, is by supposing the fluoric acid, like muriatic acid, composed of hydrogen, and a substance, as yet unknown, in a separate form possessed, like oxygen and chlorine, of the negative electrical energy, and hence determined to the positive surface, and strongly attracted by metallic substances.

This view is much more conformable to the general order of chemical and electrical facts than the third hypothesis, just now mentioned.

It is indeed possible to conceive, if the metals be regarded as compounds of hydrogen, that the hydrogen may be produced from the metal positively electrified at the time that the acid combines with its supposed basis, and that this hydrogen may be transferred to the negative surface; but this supposition involves a multitude of others; and the results of the electrization of fluoric acid are analogous to most of the results of the electrization of water and muriatic acid, both of which are shown by analysis and synthesis to be compounds of hydrogen; and in the electrical decomposition of these bodies, their characteristic element is generally combined with the positive metallic surface.

In the Bakerian Lecture for 1810, I have given an account of the action of potassium upon pure silica. In this process, the potassium acquires oxygen, and a combustible substance, which consists either of the basis of silica, or the basis of silica combined with potassium appears. In supposing the silicated fluoric acid gas to be composed of this basis and the fluoric principle, it is easy to explain the action of potassium upon it, and the complicated phenomena, occasioned by the agency of water, and acids, and oxygen, on the results of this action. The potassium must be conceived to attract a part of the fluoric principle from the siliceous basis, or to form a triple compound, from which silicated fluoric acid gas is capable of being reproduced, in consequence of the combination of a part of the potassium and siliceous basis with oxygen; and on this idea the cause of the apparent loss of the fluoric principle, in the experiments on the action of ammonia on the product of the combustion of potassium in silicated fluoric acid gas, becomes obvious.

Assuming then from the analogy with chlorine, that the different fluoric compounds consist of inflammable bodies united to a peculiar principle, it follows that all attempts to decompose the fluoric acids, by combustible substances, can lead to no other result, than that of occasioning new combinations of the fluoric principle; and the only methods which seemed plausible for obtaining this principle pure, after that by electrical decomposition had failed, were by the action of oxygen or chlorine on certain of its compounds. Chlorine is, in certain instances, detached from hydrogen by oxygen; and oxygen, in a number of cases, is detached from metals by chlorine; I thought it therefore probable, that the fluoric principle might, in some process, be separated from bases by either chlorine or oxygen.

In selecting compounds for experiments of this kind, I was guided by the relative attractions of the fluoric and muriatic acids, of chlorine and oxygen. Horn-silver and calomel, and muriate of potassa are not decomposed by fluoric acid, but fluate of silver, of mercury, and of potassa are easily decomposed by muriatic acid; I therefore conceived, that the fluoric principle would most likely be expelled from the dry fluates of silver, mercury, and potassa by chlorine.

I made some pure fluates of silver and mercury, by dissolving the oxides of these metals in fluoric acid, and I heated them in small trays of platina; much fluoric acid was driven off in this process, which I continued in the case of the fluate of mercury till the salt began to sublime, and in that of the fluate of silver till it was red hot.

The dry salts were introduced in small quantities into glass retorts, which were exhausted and then filled with pure chlorine: the part of the retort in contact with the salt was heated gradually till it became red. There was soon a strong action, the fluate of mercury was rapidly converted into corrosive sublimate, and the fluate of silver more slowly became horn-silver. In both experiments there was a violent action upon the whole of the interior of the retort. On examining the results, it was found that in both instances there had been a considerable absorption of chlorine, and a production of silicated fluoric acid gas, and oxygen gas.

I tried similar experiments, with similar results, upon dry fluates of potassa and soda. By the action of a red heat, they were slowly converted into muriates with the absorption of chlorine, and the production of oxygen, and silicated fluoric acid gas, the retort being corroded even to its neck.

The obvious explanation of these phenomena is, that a particular principle, the acidifying matter of the fluoric acid, combined with the metals, is expelled from them by the stronger attraction of the chlorine, and that this principle coming in contact with glass decomposes it by its attraction for the silicum and sodium, and separates them from the oxygen with which they were combined.

I made various attempts to procure the fluoric principle in a pure form. I heated the fluates of potassa and soda in trays of platina, in a tube of platina connected with a vessel filled with chlorine. In this case the fluates were converted into muriates, with a considerable increase of the weight of the tray; and the platina was violently acted upon, and covered with a reddish brown powder; and in the instance in which fluate of potassa was used, a compound of fluate of platina and muriate of potassa was formed.

There was a considerable absorption of chlorine; but no new gaseous matter could be discovered in the gas in the tube.

I tried to obtain the fluoric principle pure, by decomposing the fluates in a tube of silver, but with no better success; the silver was acted upon both by the chlorine and the fluoric principle, and rapidly dissolved. I used glass tubes coated with resin of copper (*cuprane*) and horn-silver (*argentane*), on which I concluded that the fluoric principle would have no action from the decomposition of fluate of silver by chlorine; but at the degree of heat required to decompose the fluoric salts, the muriates were always fused, the glass violently acted upon, and silicated fluoric acid gas formed.

In one instance, in which fluate of potassa had been heated in a platina tray and tube, in which muriate of potassa had been fused, for the purpose of defending the interior, as much as possible, from the action of the fluoric principle, the gas, when disengaged into the atmosphere, had a peculiar smell, different from that of chlorine, (which certainly formed the greatest proportion of the elastic matter,) and more disagreeable; and dense white fumes were produced by its action upon the air. A portion of this gas thrown into a glass receiver, over mercury, acted upon the glass, and silicated fluoric acid gas was generated. On examining the platina tray, however, it was found corroded, and the reddish brown powder formed.

In the course of these investigations, I made several attempts to detach hydrogen from the liquid fluoric acid, by the agency of oxygen and chlorine. It was not decomposed when passed through a platina tube heated red with chlorine, nor by being distilled from salts containing abundance of oxygen, or those containing abundance of chlorine.

I distilled the fluates of lead and mercury with phosphorus and sulphur, with the hope of obtaining compounds of the fluoric principle with phosphorus and sulphur. In all experiments of this kind, a decomposition took place, and the glass tubes employed were violently acted upon, and sulphurets and phosphurets were formed. When I used tubes lined with sulphur the decomposition was less perfect; but minute quantities of limpid fluid condensed in a part of the tube cooled by ice, both in the cases when sulphur and when phosphorus were used; it had the appearance of hydrofluoric acid, and speedily dissipated itself in white fumes. Whether they were that substance which had obtained its hydrogen from these inflammable bodies, or compounds of sulphur and phosphorus with the fluoric principle, I have not ascertained, but the first opinion seems most probable.

When I heated fluate of lead and finely powdered charcoal strongly in the air, the lead became revived, and white fumes were produced. I thought it probable, that in this case a compound of fluorine and charcoal was formed, but on trying the experiment in a close vessel of platina, no change took place; and it evidently depended upon the presence of hydrogen in the vapour of the atmosphere, or in the flame of the spirit lamp, by which the experiment was made, and I found muriate of silver decomposed, and silver produced under the same circumstances.

From the general tenour of the results that I have stated, it appears reasonable to conclude that there exists in the fluoric compounds a peculiar substance, possessed of strong attractions for metallic bodies and hydrogen, and which combined with certain inflammable bodies forms peculiar acids, and which, in consequence of its strong affinities and high decomposing agencies, it will be very difficult to examine in a pure form, and, for the sake of avoiding circumlocution, it may be denominated *fluorine*, a name suggested to me by M. Ampere.

From experiments that I have made on the composition of the fluoric combinations, and which I shall soon have the honour of communicating to the Society, it appears that the number representing the definite proportion in which fluorine combines, is less than half the number representing that in which chlorine combines; and hydrates in becoming fluates lose weight, so that on the generally received idea of the existence of a peculiar acid in the fluates, and of their being compounds of oxides, with an acid containing oxygen, that acid, according to the law of definite proportions, must contain more oxygen in proportion to its quantity of inflammable matter than water, which is highly improbable, and contrary to all analogies.

Dr. Wollaston has found, that the fluoric combinations have very low powers of refracting light, and particularly the pure fluoric acid; so that the refracting powers of fluorine will probably be found lower than those of any other substance, and it appears to possess higher acidifying and saturating powers than either oxygen or chlorine.

It is easy to perceive, in following the above theory, that all the ideas current in chemical authors respecting the fluoric combinations, must be changed. Fluor spar, and other analogous substances, for instance, must be regarded as binary compounds of metals and fluorine.

Many objects of inquiry arise, likewise, from these new views: the topaz contains the fluoric principle, but new experiments are required to shew whether that gem is a true silicated fluate of alumina, or a compound of the inflammable bases of alumina and silica with fluorine.

I have ascertained that the chrysolite yields no silicated fluoric gas, when acted on by sulphuric acid, but merely pure fluoric acid; but I have not continued the research so far, as to determine whether it contains fluorine united to inflammable matter only, or fluorine and oxygen.

XV.

AN ACCOUNT OF SOME NEW EXPERIMENTS ON THE FLUORIC COMPOUNDS; WITH SOME OBSERVATIONS ON OTHER OBJECTS OF CHEMICAL INQUIRY.*

In this paper I shall offer to the society a continuation of those researches, the details of which have been already honoured with a place in their Transactions; and I trust, that the experiments and observations which I have to communicate, will be found to elucidate some important but obscure parts of chemical philosophy.

In the last paper, which I had the honour of presenting to this body, I have given an account of a number of experiments made with a view of decomposing the fluoric acid: the most probable inference, from my results, was that the pure liquid fluoric acid consists of hydrogen united to a substance, which, from its strong powers of combination, has not as yet been procured in a separate form, but which is detached from hydrogen by metals, and which, in union with the basis of the boracic acid and silica, forms the fluo-boric and silicated fluoric gases.

All the new experiments, that I have made on the fluoric compounds, tend to confirm this idea; and the various attempts that I have made, since the last session, to decompose the principle in the fluoric acid separated at the negative surface in Voltaic combinations, have been unsuccessful.

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* [From the Philosophical Transactions for 1814; Read before the Royal Society, February 13, 1814.]

I have found that fluate of lead, the substance formed by the action of a soluble salt of lead on fluate of potassa or fluate of ammonia, is immediately decomposed, when moist, by ammoniacal gas, and a white powder separates from it, which must contain oxygen, as it gives carbonic acid by being ignited with charcoal; but dry fluate of lead may be fused in ammonia without undergoing the slightest alteration, and the presence of water, which may furnish oxygen to the lead and hydrogen to the fluoric principle, seems absolutely necessary for the production of oxidated bodies from the fluoric combinations.

I obtained analogous results by acting on silicated fluate of ammonia and fluo-borate of ammonia by chlorine; when the salts were moist, or when the gas was not free from vapour, silica and boracic acid were formed in small quantities, but when water was carefully excluded, these bodies did not appear; and the results were muriate of ammonia and silicated fluoric gas, or fluo-boric gas and azote.

I ignited two points of charcoal intensely in fluo-boric and silicated fluoric acid gases, pure, and mixed with oxygen; but no change, indicating a decomposition, took place; the only new product was a little inflammable gas, which was probably disengaged from the charcoal.

I passed pure liquid fluoric acid over charcoal ignited to whiteness, in a platinum tube; no carbonic acid was formed, and a very minute quantity of gas only was produced, which proved to be hydrogen.

I mentioned in my last communication to the society, that I made several experiments on the composition of the fluates; since that time I have repeated some of the processes, and I shall detail such of the results as appear to be most correct. In experiments on the fluates, it is very difficult to exclude sources of inaccuracy; glass vessels cannot be used, and even silver and platinum vessels are slightly acted upon by the pure fluoric combinations soluble in water.

Fluor spar decomposed by sulphuric acid increases more than three-fourths of its original weight; but to produce this result several successive distillations of it with fresh portions of acid are necessary, and the spar must be in very fine powder, and the result must be powdered after every operation.

In an experiment made in a crucible of platinum, in which very pure white Derbyshire spar was used, and sulphuric acid distilled in glass vessels, and in which the product was heated to whiteness in every operation, 100 grains became in the first operation 159·4 grains, which gained

in the second . . .	6·9
in the third	4·2
in the fourth . . .	1·9
in the fifth	1·6
in the sixth	0·9
in the seventh . . .	0·3
in the eighth	·0
Total increase	75·2

If this result be calculated upon, supposing the number representing calcium to be 40, as I have given it in my Elements of Chemical Philosophy, the number representing fluorine will be 34·2, and fluor spar must be supposed to be composed of 40 calcium, and 34·2 fluorine.

22 grains of fused subcarbonate of potassa decom-

posed by diluted liquid fluoric acid; in an experiment made with great care, was found to afford 18·15 grains of dry fluate of potassa; and by a very careful analysis, it was found that this subcarbonate contained 31 per cent. of carbonic acid; now, if the remainder of the salt be supposed to be pure potassa, and the calculations be made on this idea, it will appear that fluate of potassa must consist of 125·9 of potassium and 54·74 of fluorine, and the number representing fluorine must be regarded as 32·6.

The 18·15 grains of fluate of potassa decomposed by sulphuric acid afforded 38·5 of acid sulphate of potash. Supposing acid sulphate of potassa to consist of two proportions of sulphuric acid 150, and one of potassa 90, they ought to have given 40 grains, and this loss is no more than might be expected in the process of decomposition and evaporation.

In some experiments that I made on the decomposition of the hydrates of potassa and soda, when decomposed by fluoric acid, the results indicated a number for fluorine a little lower. Thus 20 grains of hydrate of potassa were converted into 19·8 grains of fluate of potassa, and 20 grains of hydrate of soda into 19·6 of fluate of soda; but I do not place so much confidence in these results, as there always was great heat produced during the action of the acid upon the hydrates; and probably a minute quantity of the hydrates might have been dissipated at the beginning of the process.

It appears reasonable to conclude, as I have stated in my last paper, published in the Philosophical Transactions, that the number representing fluorine is less than half of that representing chlorine, about 33.

I endeavoured to ascertain the composition of fluate of ammonia, by adding together hydro-fluoric acid and solution of ammonia of known composition, and I found in this way, that 100 parts of solution of ammonia of specific gravity 9162 required for its saturation 52 grains of diluted fluoric acid of such a strength, that an equal portion produced exactly 32 grains of fluate of potassa. According to this experiment fluate of ammonia must consist of 9·7 of fluorine to 22 of ammonia;* and, if it be conceived that liquid fluoric acid consists of two proportions of hydrogen to one of fluorine, the true composition of fluate of ammonia will be one proportion of fluoric acid 35, and two proportions of ammonia 64.†

The volatility of fluate of ammonia rendered it impossible to ascertain by evaporation the real quantity of solid salt formed, though the heat was never raised so high as that of boiling water, yet only 12·7 grains of solid fluate of ammonia could be procured.

Two hundred cubical inches of ammonia, which weigh 36 grains, condense one hundred cubical inches of silicated fluoric gas, weighing, under the same circumstances, 110·7, and if it be supposed that silicated fluate of ammonia contains one proportion of silicated fluoric acid and one of ammonia, then the number representing silicated fluoric gas will be 98·4, and it may be conceived to consist of two proportions of fluorine 66, and one of the siliceous basis 32·4.

According to the experiments of John Davy, 100 parts of silicated fluoric acid afford 61·4 of silica when decomposed by aqueous solution of ammonia, hence silica may be conceived to consist of 32·4 of basis and 29 of oxygen, and this is very near two proportions of oxygen.

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* See Elements of Chemical Philosophy, p. 198.

† That is, if ammonia be regarded as composed of one proportion of azote 26, and six of hydrogen.

I decomposed 20 grains of silicated fluate of ammonia by solution of ammonia, and I obtained 9·2 grains of silica. This result offers proportions very little different from those gained in the preceding calculations.

I have made some direct experiments with the hopes of determining the proportions of oxygen in silica, but they have been unsuccessful; I have ascertained, however, that it requires more than three parts of potassium to decompose one part of silica, which shews that this substance cannot contain much less than half its weight of oxygen.

I have endeavoured to separate the siliceous basis in a pure form, with the view of making synthetical experiments on its nature by combustion in oxygen, and my results, though not perfectly satisfactory, yet seem worthy of notice, and may lead to more successful attempts.

I decomposed silica by passing potassium in excess through it, in a heated tube of platinum: the result consisted chiefly of alkali containing a dark coloured powder, the basis of silica diffused through it. I fused the whole mass with sulphur, which, in combining with the dry alkali, produced ignition. I attempted to detach the sulphuret of potassa by water: in this case the dark particles separated, but during their separation and after, they acted upon the water of the solution producing gas, and in attempting to collect them by the filter, I failed to procure sufficient for examination, for they were principally converted into silica.

I heated the substance procured in another experiment of this kind with hydrate of potassa; in this case there was a copious effervescence, and silica appeared to be reproduced and dissolved by the alkali.

I heated a portion of a similar result in strong lixivium of potassa; the solution gained a tint of olive, but there was scarcely any effervescence; from this it seems probable, that the inflammable basis of silica, like boron, is soluble in alkaline solutions without decomposing them.

Indeed this body, in its general characters, appears very analogous to boron. It appears to be neither volatile nor fusible; its oxide exerts, like boracic acid, a neutralizing power on the alkalies, though of a feebler kind, and forms, like boracic acid, vitreous bodies with the alkaline earths, and, like boron, the siliceous basis in combination with fluorine constitutes a powerful acid.*

In my first views of the nature of the boracic and siliceous bases, I thought it probable that they would both appear as metals, if they could be entirely freed from oxygen: but it now seems more probable, that they form a class by themselves, offering a kind of link in the chain of natural bodies, when arranged according to their analogies, between charcoal, and sulphur and phosphorus.

It seems worthy of an experimental inquiry, whether the siliceous basis may not be obtained pure by heating the result procured from silica by potassium with pure sulphuric acid, which might possibly detach the potassa to form acid sulphate of potassa, without being decomposed by the inflammable basis.

I have made many new experiments with the hope of decomposing chlorine, but they have been all unavailing; nor have I been able to gain the slightest evidence of the existence of that oxygen which many persons still assert to be one of its elements.

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* [This view has been confirmed by the later researches of Berzelius. In the purest state in which he obtained the basis of silica, he found it in many of its properties analogous to boron, of a brown colour, without metallic lustre, and a non-conductor of electricity; and what is very curious, he found that it is little disposed to combine with oxygen to form silica, unless an alkali is present and heat applied.—Vide Annals of Philosophy, vol. xxvi.]

I kept sulphuret of lead for some time in fusion in chlorine, the results were sulphurane (Dr. Thomson's liquor) and plumbane (muriate of lead); not an atom of sulphate of lead was formed in the experiment, though if any oxygen had been present, this substance might have been expected to have been produced.

I heated plumbane (muriate of lead) in sulphurous acid gas, and likewise in carbonic acid gas, but no change was produced; now, if oxygen had existed either in chlorine, or in its combination with lead, there is every reason to believe, that the attractions of the substances concerned in these experiments would have been such as to have produced the insoluble and fixed salts of lead, the sulphate in the first case, and the carbonate in the second.

I shall not enter into any discussion upon the experiments in which water is said to be produced by the action of muriatic gas on ammonia: there is, I believe, no enlightened and candid person, who has witnessed the results of processes in which large quantities of muriate of ammonia, made by the combination of the gases in close vessels, have been distilled, without being satisfied, that there is no more moisture present, than the minute quantity which is known to exist in the compound vapours diffused through ammoniacal and muriatic acid gases, which cannot be considered either as essential to the existence of the gases, or as chemically combined with them.*

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* Dr. Henry found it very difficult to free ammonia from the aqueous vapour existing in it by hydrate of potassa, and probably the hydrated muriatic vapour which I have detected in muriatic acid gas, by a freezing mixture, is not decomposable by muriate of lime.

One of the first experiments that I made, with the hope of detecting oxygen in chlorine, was by acting upon it by ammonia, when I found that no water was formed, and that the results were merely muriate of ammonia and azote;* and the driest muriate of ammonia, I find, when heated with potassium, converts it into muriate of potassa, which result would be impossible on the hypothesis of oxymuriatic gas being a compound of oxygen, for, if there was a separation of water during the formation of the muriate, the same oxygen could not be supposed to be detached in water, and yet likewise to remain so as to form part of a neutral salt.

If water had been really formed during the action of chlorine on ammonia, the result would have been a most important one: it would have proved either that chlorine or azote was a compound, and contained oxygen, or that both contained this substance; but it would not have proved the existence of oxygen in chlorine, till it had been shewn that the azote of the ammonia was unchanged in the operation.

Some authors continue to write and speak with scepticism on the subject, and demand stronger evidence of chlorine being undecompounded. These evidences it is impossible to give. It has resisted all attempts at decomposition. In this respect, it agrees with gold, and silver, and hydrogen, and oxygen. Persons may doubt, whether these are elementary bodies; but it is not philosophical to doubt, whether they have not been resolved into other forms of matter.

By the same mode of reasoning, as that in which oxygen is conceived to exist in chlorine, any other species of matter might be supposed to form one of its constituent parts; and by multiplying words all the phenomena might be satisfactorily explained. Thus in the simple view of the formation of muriatic acid, it is said one volume of chlorine combines with one of hydrogen, and they form two volumes of muriatic acid gas. In the hypothesis of chlorine containing oxygen, it is said, the oxygen of the chlorine combines with the hydrogen to form water, and this water unites to an unknown something, or dry muriatic acid, to produce a gaseous body. If it were asserted that chlorine contained azote, oxygen, and this unknown body, then it might be said, that, in the action of hydrogen on chlorine, the azote, the oxygen and the chlorine, having all attractions for hydrogen, enter into union with it, and form a quadruple compound.

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* [Page 288, of this Vol.]

Professor Berzelius has lately adduced some arguments, which he conceives are in favour of chlorine being a compound of oxygen from the laws of definite proportions; but I cannot regard these arguments of my learned and ingenious friend as possessing any weight. By transferring the definite proportions of oxygen to the metals, which he has given to chlorine, the explanation becomes a simple expression of facts; and there is no general canon with respect to the multiples of the proportions in which different bodies combine. Thus azote follows peculiar laws in combining with every different body; it combines with three volumes of hydrogen, with half a volume of oxygen, with 1·2 and 1½ of the same body, and with four volumes of chlorine.

The chemists in the middle of the last century had an idea that all inflammable bodies contained phlogiston or hydrogen. It was the glory of Lavoisier to lay the foundations for a sound logic in chemistry, by shewing that the existence of this principle, or of other principles, should not be assumed where they could not be detected.

In all cases, in which bodies support combustion or form acids, oxygen has been supposed by the greater number of modern chemists to be present; but as there are many distinct species of inflammable bodies, so there may be many distinct species of matter which combine with them with so much energy, as to produce heat and light; and various bodies appear capable of forming acids; thus hydrogen enters into the composition of nearly as many acids as oxygen, and three bodies, namely, sulphuretted hydrogen, muriatic acid, and fluoric acid, which contain hydrogen, are not known to contain oxygen. The existence of oxygen in the atmosphere, and its action in the economy of nature, and in the processes of the arts, have necessarily caused it to occupy a great portion of the attention of chemists, and, being of such importance, and in constant operation, it is not extraordinary, that a greater number of phenomena should be attributed to it, than it really produces.

In the views that I have ventured to develope, neither oxygen, chlorine, nor fluorine, are asserted to be elements; it is only asserted, that, as yet, they have not been decomposed.

As the investigation of nature proceeds, it is not improbable, that other more subtile bodies belonging to this class will be discovered, and perhaps some of the characteristic differences of those substances, which apparently give the same products by analysis, may depend upon this circumstance.

The conjecture appears worth hazarding, whether the carbonaceous matter in the diamond may not be united to an extremely light and subtile principle of this kind, which has hitherto escaped detection, but which may be expelled, or newly combined, during its combustion in oxygen. That some chemical difference must exist between the hardest and most beautiful of the gems and charcoal, between a non-conductor and a conductor of electricity, it is scarcely possible, notwithstanding the elaborate experiments that have been made on the subject, to doubt: and it seems reasonable to expect, that a very refined or perfect chemistry will confirm the analogies of nature, and shew that bodies cannot be exactly the same in composition or chemical nature, and yet totally different in all their physical properties.

XVI.

SOME EXPERIMENTS AND OBSERVATIONS ON A NEW SUBSTANCE WHICH BECOMES A VIOLET-COLOURED GAS BY HEAT.*

A NEW and a very curious substance has recently occupied the attention of chemists at Paris.

This substance was accidentally discovered about two years ago by M. Courtois, a manufacturer of saltpetre

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* [From the Philosophical Transactions; Read before the Royal Society, January 20, 1814.

This paper on Iodine was the first he communicated after leaving England. The author's love of research and his zeal in prosecuting it were strongly shewn by his labours whilst on the Continent, especially taking into consideration the very limited means of experimenting which he then commonly had at his disposal, and which were comprised in the contents of two small boxes, now in my possession, one 20 inches long, 7 wide, and 4 high, used for holding tests,—the other 12 inches long, 7¼ wide, and 6 high for holding instruments,—as glass tubes, small receivers, retorts, and capsules, — a blow-pipe apparatus, a small pneumatic trough, a delicate balance, and a few other necessary articles. The balance, on account of its usefulness, for the purposes of the traveller, may be deserving of particular mention: it was constructed entirely of platina and of agate,—the knife-edge and other parts subject to friction of the latter material,—the rest of the metal; and consequently was not subject to rust in whatever way used or exposed. Limited as these means were he held them to be sufficient: in the "Consolations in Travel," he expresses such an opinion,—he says that for the wants of the philosophical chemist, "all the implements absolutely necessary may be carried in a small trunk," and he gave proof of the same, by the inquiries which he carried on with them, and the new facts he brought to at Paris. In his processes for procuring soda from the ashes of sea weeds, (cendres de varec) he found the metallic vessels much corroded; and in searching for the cause of this effect, he made the discovery. The substance is procured from the ashes, after the extraction of the carbonate of soda, with great facility, and merely by the action of sulphuric acid:—when the acid is concentrated, so as to produce much heat, the substance appears as a vapour of a beautiful violet colour, which condenses in crystals having the colour and the lustre of plumbago.

M. Courtois, soon after he had discovered it, gave specimens of it to MM. Desormes and Clement for chemical examination; and those gentlemen read a short memoir upon it, at a meeting of the Imperial Institute of France, on November 29th. In this memoir, these able chemists have described its principal properties; they mentioned that its specific gravity was about four times that of water, that it becomes a violet-coloured gas at a temperature below that of boiling water, that it combines with the metals and with phosphorus and sulphur, and likewise with alkalies and metallic oxides, that it forms a detonating compound with ammonia, that it is soluble in alcohol, and still

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light, only occasionally in particular researches having recourse for aid to the well appointed laboratories established in Florence and Rome. In this respect, doing much with small means, he followed the example of all the illustrious discoverers who preceded him, especially Black, Scheele, and Priestley—it is not therefore extraordinary,—and I should not have thought it necessary to call attention to the circumstance, were it not for the remark occasionally made by persons not well acquainted with the history of chemistry, and who probably have not reflected much on the powers of the human mind, and what the inventive faculty is capable of, that he was in great measure indebted for his success to his opportunities, such as he had the benefit of, and which he turned to so good an account at the Royal Institution.]

more soluble in ether; and that by its action upon phosphorus and upon hydrogen, a substance having the characters of muriatic acid is formed. In this communication they offered no decided opinion respecting its nature.

M. Ampere had the goodness to give me some of this substance, and M. Clement having requested me to submit it to some analytical tests, I made several experiments upon it, which convinced me that it was a new substance undecompounded in any of the circumstances to which I was able to expose it; and that the acid formed in processes upon it was not muriatic acid, but a new acid possessing a striking resemblance to that body.

M. Gay Lussac (to whom M. Clement had furnished some of the substance, and with whom he had made some experiments upon it before the communication of his memoir) on Monday, Dec. 6, read to the Institute a paper, in which he stated that the acid formed by its action on hydrogen is a peculiar one. He mentioned several interesting particulars respecting the mode of its production, and he compared it to oxymuriatic gas or chlorine, and stated that two hypotheses might be formed on its nature; and that it might be considered as an undecompounded substance, or as a compound of oxygen. M. Gay Lussac is still engaged in experiments on this subject, and from his activity and great sagacity, a complete chemical history of it may be anticipated. But as the mode of procuring the substance is now known to the chemical world in general, and as the combinations and agencies of it offer an extensive field for inquiry, and will probably occupy the attention of many persons, and as the investigation of it is not pursued by the discoverer himself, nor particularly by the gentlemen to whom it was first communicated, I shall not hesitate to lay before the Royal Society an account of the investigations I have made upon it; and I do this with the less scruple, as my particular manner of viewing the phenomena has led me to some new results, which probably will not be considered by the Society as without interest in their relation to the general theory of chemistry, and in their possible application to some of the useful arts.

The first experiments that I made on this substance, were to ascertain whether (argentane) muriate of silver could be formed from its solution in water or alcohol, and for this purpose it was purified by distilling it from lime. Its solution, I found, when mixed with solution of nitrate of silver, deposited a dense precipitate, of a pale lemon-colour; this precipitate when collected and examined, proved to be fusible at a low red heat, and then became of a red colour. When acted upon by fused hydrate of potassa, it was rapidly decomposed, and a solid substance, having all the characters of oxide of silver, was formed. The matter soluble in water separated by a filter, and, acted upon by sulphuric acid, afforded the peculiar substance.

A solution of potassa, after being boiled on the precipitate, afforded the peculiar substance, when treated by the same acid.

The precipitate was much more rapidly altered by exposure to light, than the muriate of silver, and was evidently quite a distinct body.

Conceiving from the action of potassa upon it, that it must be a compound of the peculiar substance and silver, I endeavoured to form it directly by the combination of the two bodies. I introduced some of the substance into the closed end of a small glass curved tube, and placed in the upper part of it some silver foil; I heated the foil nearly to redness, and then passed the substance over it in vapour; there was an immediate action, the silver was rapidly dissolved, and a fusible substance formed, in all its obvious sensible and chemical characters, the same as that obtained from solutions of the substance by nitrate of silver.

The modes which occurred to me, as most likely to effect its decomposition by chemical agents, were the action of the highly inflammable metals upon it which unite to oxygen and chlorine, or the action of chlorine which in general tends to the expulsion of oxygen, and to the separation of inflammable bases from that principle.

I heated some potassium in a little glass tube, and passed some of the substance in vapour over it; at the moment the vapour came in contact with the potassium, there was an inflammation, and the potassium burnt slowly with a pale blue light. There was no gas disengaged when the experiment was repeated in a mercurial apparatus.

The substance formed by the action of potassium was white, fusible at a red heat, and soluble in water. It had a peculiar acrid taste. When acted upon by sulphuric acid, it effervesced, and the peculiar substance appeared.

It was evident that in this experiment there had been no decomposition of the body; the result seemed to depend merely upon the combination of it with the potassium.

I exposed the body to the action of chlorine in a small glass tube; it absorbed the chlorine, and a substance formed which was volatile by heat, and which appeared as a yellow solid; it was soluble in water, and rendered the water of a yellowish-green colour and strongly acid, the solution when acted upon by solution of potassa not in excess effervesced, and afforded the peculiar substance.

The acid formed by the solution of the substance united to chlorine reddened vegetable blues by its immediate contact, and soon after destroyed them.

When the new substance was heated in oxygen gas, or brought in contact with red hot hyperoxymuriate of potassa, it seemed to undergo no change.

MM. Desormes and Clement had stated, that, when the substance is combined with the metals, metallic oxides could be obtained from the solutions; I suspected that this depended upon the presence of moisture, or upon oxygen derived from the air, and experiment justified my suspicion.

I heated the substance with iron, mercury, tin, zinc, and lead, out of the contact of air; it united to them without any violence of action, and formed compounds fusible at a moderate heat, and volatile at a higher temperature. All the compounds, except that of zinc, which was white, were coloured of different shades of red-brown, red and orange; the compound it formed with tin was of a deep orange, that with iron of a bright red brown, that with lead a bright orange, that with mercury an orange still more approaching to red, and which, when crystallized, was bright crimson.

The compound of iron and the substance, when exposed to an alkaline solution, immediately deposited black oxide of iron; but when I heated it in a small retort, containing pure ammoniacal gas, no such change occurred, and it combined with the ammonia and formed a compound which volatilized without leaving any oxide.

The crimson compound of the substance with mercury united in the same manner without decomposition to potassa, and by the action of sulphuric acid, sulphate of potassa was formed, and the compound of the substance with mercury disengaged.

When the substance is made to act upon phosphorus, the two bodies combine with great rapidity at common temperatures, producing heat without light; small quantities of a strongly acid gas generally arise from the mixture, and by the application of heat, it is produced in greater quantities. When the substance is in excess, an easily fusible and volatile compound of a red colour is obtained; when the phosphorus is in excess, the greater part of the product is more fixed.

I examined the gaseous acid formed by the action of phosphorus with attention. It gives dense white fumes by combining with the aqueous vapour in the air. It has a smell very similar to that of the solid compound of chlorine and phosphorus, which itself is very analogous to that of muriatic acid. It is rapidly absorbed by water. When made to act upon ammonia, it forms with it a dense white salt, which, when acted upon by sulphuric acid, affords the peculiar substance, and at the same time a smell of hydrogen is perceived. When mercury is heated in the acid gas, the same compound as that produced by the action of the new substance directly upon mercury is formed, and hydrogen equal to half the volume of the gas is disengaged. When potassium is made to act upon it, there is no inflammation as in muriatic acid gas, but the potassium becomes converted into a body, similar to that produced by its combustion in the vapour of the substance, and a gas equal to half the volume of the acid gas, which burns in the same manner as hydrogen, is disengaged.

When the easily fusible and volatile compound of the substance with phosphorus is heated in water, it rapidly dissolves in it, and forms a strong acid, which, when evaporated, leaves hydrophosphorous acid, and which, before its evaporation, neutralized by potash, and acted on by sulphuric acid, affords the peculiar substance.

When the difficultly fusible substance it forms with phosphorus is acted on by a small quantity of water, and heated in a glass tube, much gas spontaneously inflammable is disengaged, and a white sublimate arises, which, when acted on by cold water, becomes hot, and affords a considerable quantity of a gas having all the properties of hydrophosphoric gas.

The solution of this crystalline substance in water, neutralized by potash, and decomposed by sulphuric acid, afforded the peculiar substance, but when the solution was heated strongly before its neutralization, it left only hydrophosphorous acid, which when heated gave off hydrophosphoric gas, and became phosphoric acid.

It is easy to explain all these phenomena, except the production of the acid gas, which is a compound of the peculiar substance and hydrogen: to account for the appearance of this body, it is necessary to suppose the existence of hydrogen, or of water in the substance, or of hydrogen in phosphorus.

I used the substance distilled through quick lime, which there is every reason to believe would absorb all the water united to it: in this case the acid gas, which gave hydrogen when decomposed by mercury, was produced in much smaller quantities; but, when the substance was moistened, the gas was afforded in very large quantities. It is probable, that a little hydrogen existing in the phosphorus, and which appears when that substance is acted on by Voltaic electricity, may influence the result; but I am inclined to attribute it principally to the moisture adhering to the substance, and I have never been able to produce more gas from the fusible compound by distilling it with a new quantity of phosphorus.

When the fusible compound of the substance with phosphorus is distilled with a small quantity of water, the gas produced seems to be of the same kind as that obtained by the action of heat during the combination, and both these gases when absorbed by water afford, when acted upon by nitrate of silver, the same product as that formed by the action of a solution of the substance in water on the same salt.

I attempted to form a compound of the substance with hydrogen directly, by heating it in several experiments to redness in a glass tube filled with hydrogen. When the gas was moist, or when the tube contained vapour, a strong acid fluid was formed of a deep yellow colour. When the gas and the substance were dry, there was an expansion of volume, and on breaking the tube, fumes appeared similar to those produced by the action of the gas formed during the union of phosphorus and the substance, and which precipitated in the same manner a solution of nitrate of silver. This peculiar acid, which consists of the substance united to hydrogen, has a very strong attraction for water, and a very small quantity of water absorbs a large quantity of the gas, and when combined with water, it rises with it in vapour, and in its state of liquid acid, it rapidly dissolves the substance, and becomes tawny.

The new substance, as MM. Desormes and Clement have shewn, is rapidly soluble in solution of potash; when it is in excess the solution becomes red brown. On evaporating the mixture and heating it to redness, a substance is formed, exactly similar to that produced by the combination of the substance with potassium.

As potassa is a compound of potassium and oxygen, it is evident that, to form a compound of potassium and the substance from potassa, oxygen must be expelled, and I found by experiment, that this was the case; and in investigating minutely the action of fixed alkaline solutions on the substance, I ascertained the existence of a class of substances, precisely similar to the hyperoxymuriates, consisting of oxygen, the substance, and potassium, and formed in a manner exactly analogous.

If the substance is thrown into a moderately strong solution of potassa, as it dissolves crystals fall down, and by saturating the solution with the substance, considerable quantities are obtained. By pouring off the mother liquor and evaporating it a little, more of the crystals fall down.

All these crystals, if precipitated from a solution not too much saturated, are of the same kind; they are little soluble in water, have a taste analogous to that of the hyperoxymuriates of potassa, scintillate when thrown upon burning coals, and form a deflagrating mixture when mixed with charcoal. When fused, they give off abundance of oxygen gas, and become the same substance as that formed by the action of potassium on the new substance.

If the liquor which has ceased to afford the crystals be evaporated to dryness, it yields a considerable quantity of a substance which is not capable of detonating with combustible bodies, and which is the same as that afforded by the combination of the substance with potassium.

It is evident then that the oxygen contained in the potassa is newly combined by the action of the new substance, and two compounds formed, one consisting of potassium and the new substance, the other of potassium, the new substance, and the oxygen contained in the potassa.

By passing the vapour of the substance over dry red hot potassa formed from potassium, oxygen is expelled, and it appears that oxygen cannot remain in the triple compound at a heat above the red heat.

By dissolving the substance in solutions of soda and baryta similar results are obtained, and in each case two compounds are formed. The oxygen is condensed in one, and the other consists simply of the new substance and a metal.

To separate entirely the deflagrating salt from the soluble salt is not easy, there always remains in the mother liquor a little of the deflagrating compound; but by separating the first crystals from solution of potassa not too strong, the deflagrating compound of oxygen, potassium, and the substance is obtained, apparently pure.

As the new substance combines with potassium and the metals with much less energy than chlorine, it occurred to me, that it would probably be expelled from its combinations by that body; and this I have found to be the case in all the experiments I have made. When the compound of the new substance and potassium is heated in contact with chlorine, potassane (muriate of potassa) is formed, the violet gas appears, but soon combines with chlorine, and they form together the peculiar acid compound I have before described; but towards the end of the process, as the proportion of chlorine diminishes, the violet coloured gas again appears.

When the compound of the substance with silver was treated in the same manner (argentane) muriate of silver was formed, and the substance combined with chlorine at the commencement of the operation, but was disengaged uncombined towards the end.

Similar phenomena occurred when the compounds of the substance with mercury and lead were acted on by chlorine.

The action of acids on the compounds of this substance, are what might be expected from its analogies to chlorine.

When concentrated sulphuric acid is poured upon the compound of the substance and potassium, some of the substance appears; but a part of it rises in combination with hydrogen and water, and condenses by cold, and appears of a deep orange colour from having dissolved some of the substance. The sulphuric acid likewise seems to retain some of the substance, for it continues red after being strongly heated, and the acid is partly decomposed, for sulphurous acid gas is disengaged.

It seems probable, that the acid is decomposed to furnish oxygen to a portion of the potassium, which quits in consequence the new substance, and that water is likewise decomposed to furnish hydrogen to another portion of the substance, and that the hydrogen and the substance, in their acid form, combine with the water of the sulphuric acid, and rise in vapour, sulphate of potassa being at the same time produced.

When sulphuric acid is poured on the salt consisting of the substance, oxygen, and potassium, the substance reappears, and there is a slight effervescence. In this case, part of the oxygen is employed to form potassa, and the remainder is expelled unaltered.

When concentrated nitric acid is made to act on the triple compound, similar phenomena occur, and the substance reappears with effervescence.

When nitric acid is applied to the double compound, there is a smell of nitrous gas, and the substance is instantly reproduced.

With concentrated muriatic acid, the phenomena presented by the two different alkaline compounds, the binary and the triple, are very interesting. When the acid is brought in contact with the triple salt, there is no effervescence, but a substance, which appears to be a compound of chlorine and the new body, is formed and dissolved in the water of the acid, and potassane is precipitated.

When the double compound is used, there is a complete solution with a partial decomposition, and by applying a gentle heat, the excess of muriatic acid is driven off, and the same acid as that procured by the action of the substance on hydrogen remains dissolved in the liquor. When mixtures of the two salts are employed, the substance itself appears.

It appears that in the instance when the triple compound is employed, there is not only sufficient oxygen to attract the hydrogen from the chlorine which is to combine with the potassium, but likewise enough to decompose a portion of muriatic acid, sufficient to afford chlorine to enter into combination with the whole of the substance.

When the binary compound is used, the result is a simple instance of double affinity; the new substance quits the potassium to unite to the hydrogen of the muriatic acid, and the chlorine and the potassium combine: and that the decomposition is only partial depends probably upon the attraction of the compound of iodine and potassium for water. When mixtures of the compounds are used, the oxygen is employed to attract hydrogen from the substance, to which it seems to adhere with a much weaker attraction than to chlorine.

MM. Desormes and Clement have mentioned, that when the new substance is exposed to liquid ammonia, a black powder is formed, which, when dry, fulminates by the slightest contact or friction. I introduced some of the substance into solution of ammonia, and separated the liquor from the black powder, and evaporated it to dryness; it left a white saline substance, the same as that produced by the union of ammonia with the peculiar acid, which consists of the substance combined with hydrogen; and hence it appeared probable that a portion of ammonia had been decomposed to furnish hydrogen.

I made the experiment on the action of strong solution of ammonia on the substance in a pneumatic apparatus; but no azote was given off. Hence I am induced to conclude, that the black powder is a compound of the new substance and azote, similar in its character of a binary compound to the detonating oil discovered by M. Dulong; and this conclusion is strengthened by the results of its detonation in a tube of glass partially exhausted: they are, I find, the peculiar substance and a gas which is not inflammable, and which does not support flame; and unless the substance is moist, I have never been able to discover any other product; but the minute quantity I have employed prevents me from being confident on this point.*

It was an object of considerable interest to ascertain the proportion in which the new substance combines, as compared with that in which the other substances that form acids by their action on inflammable bodies enter into union.

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* [This conclusion has been confirmed by later research; from the experiments of M. Colin it appears that the compound of iodine and azote consists of one proportion of the latter and of three of the former.]

I made several experiments on this subject. 4 grains of hydrate of potassa, I found, were saturated by 6·25 of the new substance, and 2·8 the quantity of potassium in 4 grains of hydrate of potassa is to 6·25 the quantity of the substance, as 75, the number representing potassium, is to 166. Again, 1 grain of hydrate of soda required 2·1 grains of the new substance for its saturation, and 1 grain of hydrate of soda contains ·578 of sodium, so that supposing the combination of the new substance with sodium to contain a double proportion of the substance, the number representing the proportion in which it combines will be nearly 160.

Two grains of the compound of the substance with sodium decomposed by sulphuric acid afforded 128 of dry sulphate of soda, and calculating on this experiment the number is 165·5.

I have made some experiments on the quantity of the substance absorbed by tin, mercury, and lead. Mercury absorbs nearly ¾ of its weight of the new body to become the crimson substance; from which it appears that it must absorb two proportions.

My experiments have been made upon quantities too small to afford very exact results; but they shew that the new substance enters into union in a quantity much more than twice as great as that of chlorine, and, considered as an element, it offers a number much higher than those of the simple inflammable bodies, and higher even than those of most of the metals.

The most correct mode of ascertaining the number representing the proportion (supposing it to be definite, as is the case with all other bodies that have been accurately examined) in which it combines, will probably be by ascertaining the specific gravity of its gaseous compound with hydrogen. This gas, as I have stated, affords only half its volume of hydrogen, and it appeared to me to neutralize an equal volume of ammonia, so that supposing it to consist of two proportions of hydrogen, and only one of the substance, that is, to be analogous to muriatic acid gas in its nature, it must be one of the heaviest elastic fluids existing.

Taking the number representing the proportion in which this new substance combines as 165, and supposing that it occupies the same volume in this gas that chlorine occupies in muriatic acid gas, 100 cubical inches of the gas will weigh at mean temperature and pressure 95·27 grains, *i. e.* supposing hydrogen in the same quantity to weigh 2·27 grains.*

I am not at present in possession of an apparatus for weighing the gas with accuracy. A particular device will be required for this purpose, as the gas cannot be preserved over mercury. It may be collected during the action of phosphorus on the moistened substance in a vessel exhausted of air; or it may be made by heating the compound of the substance and potassium in muriatic acid gas in a glass vessel: in this case, there is, I find, a double decomposition, the chlorine quits the hydrogen to unite to the potassium, and the substance quits the potassium to unite to the hydrogen.

The new substance, I find, is not decomposed when Voltaic sparks are taken in it in its gaseous state from ignited points of charcoal: at first there are white fumes, probably from the action of moisture or hydrogen in charcoal, on the substance; but these fumes soon cease, and when the tube in which the experiment is made is cooled, the substance appears unaltered.

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* [According to the estimate of M. G. Lussac, 100 cubic inches of this gas weigh 135·988 grs; assuming as above, that it is composed of equal volumes of iodine-vapour and of hydrogen; and having found that 50 cubic inches of the former are equal to 134·92 grains.]

From all the facts that have been stated, there is every reason to consider this new substance as *an undecompounded body*. In its specific gravity, lustre, the high number in which it enters into combination and colour, it resembles the metals; but in all its chemical agencies it is more analogous to oxygen and chlorine; it is a non-conductor of electricity, and possesses, like these bodies, the negative electrical energy with respect to metals, inflammable and alkaline substances, and hence when combined with these substances in aqueous solution and electrized in the Voltaic circuit, it separates at the positive surface; but it has a positive energy with respect to chlorine, for when united to chlorine in the compound acid I have described, (page 442), it separates from the chlorine at the negative surface. This likewise corresponds with their relative attractive energy. Chlorine expels the new substance from all its combinations on which I have made any experiments.

The new substance seems to possess a stronger attraction for most of the metals than oxygen; but it is expelled from phosphorus and sulphur by oxygen: I found by passing oxygen and the compound of it with phosphorus through a glass tube heated red, phosphorous acid was formed, and the violet gas appeared.

That it produces so little heat and so seldom light in entering into combination, may be accounted for from its solid form and its great weight as an element. Potassium, however, as I have mentioned, burns in the violet-coloured gas, and when this gas is thrown upon the flame of hydrogen, it seems to support its combustion.

The saturating or neutralising powers of the new substance appear to be greater than those of oxygen, and less than those of chlorine.

It agrees with chlorine and fluorine in forming acids with hydrogen, and it agrees with oxygen in forming an acid with chlorine.

In my first experiments I conceived that it formed substances analogous to alkalies in combining with the alkaline metals, for the compound produced by its action upon solution of potassa, even when the substance was in great excess, reddened turmeric paper, and rendered green paper tinged with the juice of violets; but I have since found that this is owing to a small quantity of subcarbonate of potassa which existed in the hydrate of potassa; and when the compound is treated with the acid the substance forms with hydrogen, and heated to redness, it loses this property; and when thus formed, its taste more resembles that of a neutral salt than of an alkali. I cannot yet say with certainty whether its compound with potassium has powers like the oxides, of neutralising those acids which it does not decompose, as in all the experiments I have made on this point I used the compound which reddens turmeric; this neutralised the phosphorous, sulphurous, and boracic acids, but the effect may possibly depend upon the undecomposed carbonate.

The name *ione* has been proposed in France for this new substance from its colour in the gaseous state, from *ἴον*, viola; and its combination with hydrogen has been named *hydroionic acid*. The name *ione*, in English, would lead to confusion, for its compounds would be called *ionic* and *ionian*. By terming it *iodine*, from *ἰώδης*, violaceus, this confusion will be avoided, and the name will be more analogous to chlorine and fluorine.

The acid it forms with hydrogen may, however, be with propriety named in our language *hydroionic acid.* I venture to propose for the acid it forms with chlorine, the name of *chlorionic acid,* and for that it forms with tin *stannionic acid.* With respect to the other compounds, they may be called as a class *iodes,* with the name of the base as *iode of mercury,* and with *proto, deuto, &c.* to signify the proportions; or if a termination to the base should be preferred, as I have proposed for the combinations of chlorine, the terminations may be in *m,* with the vowels in their usual order to signify proportions. Thus, phosphoroma would signify the combination of one proportion of iodine with phosphorus, and phosphoromè would signify two proportions of iodine to one of phosphorus.

If this last plan, which involves no theoretical views, should be adopted, it may be extended with different consonants to the combinations of *fluorine,* and the vowel may be made to signify the proportion, and the consonant the nature of the compound. The vowel of termination, to the Latin name of the base, I have already proposed, on another occasion, for the compounds of oxygen. Thus argenta may be made to signify the protoxide of silver, and ferrè the deutoxide of iron. *n* is the consonant which I have suggested to represent the combination of chlorine, as *argentana* the *proto-chloride* of silver; and *l* in this system may represent fluorine. Thus *calcala* would be fluor spar, or one proportion of fluorine, and one of calcium, and the different combinations of calcium with the supporters of combustion with oxygen, fluorine, chlorine, and iodine in one proportion, would be thus expressed *calca, calcala, calcana,* and *calcama.*

I throw out these hints for discussion, rather than with any wish for their adoption, and for the purpose of directing the attention of chemists towards the subject of nomenclature, which ought to be settled on some fixed principles; and in naming a new class of compounds, great caution should be used to prevent the necessity of alteration.

In my last paper, presented to the Society two months ago, I ventured to suggest that it was probable, that new species of matter, which act with respect to inflammable bodies, like oxygen, chlorine, and fluorine, would be discovered. I had not hoped, at that time, to be able so soon to describe the properties of a body of this kind, which forms an acid with hydrogen, like chlorine and fluorine, and which in some of its combinations resembles oxygen.

This new fact will, I hope, do something towards settling the opinion of chemists respecting the nature of acidity, which seems to depend upon peculiar combinations of matter, and not on any peculiar elementary principle.

It is probable that *iodine* will be found in many combinations in nature. We may expect that it will be discovered in various marine vegetables, and in sea water, and probably the loss of weight indicated in the analysis of certain fossil substances may depend upon its expulsion.

Its compounds with the metals will probably form a new class of pigments, and it is not impossible, that the triple salts it forms containing oxygen, may be made substitutes for nitre in the manufacture of gunpowder.

Paris, Dec. 10, 1813.

XVII.

FURTHER EXPERIMENTS AND OBSERVATIONS ON IODINE.*

I. *On the triple Compounds containing Iodine and Oxygen.*

1. In this communication I shall have the honour of presenting to the Royal Society, a continuation of the inquiries I have made respecting the chemical agencies of iodine, and the properties of certain of its compounds.

2. I described in my last paper the action of iodine on the fixed alkaline lixivia, and the deflagrating salts it forms. In the first experiment which I made on these compounds, I employed the first crystals which fall down from moderately strong solutions of potassa and soda saturated with iodine, which had been purified by being repeatedly acted upon by distilled water: I now find that this process is not sufficient to free the triple compound from the double compound; and that to obtain them in a state of absolute purity, it is necessary to boil them repeatedly in small quantities of alcohol of specific gravity of from 8·6 to 9·2, which dissolves the double compound, but has little power of action on the triple compound.

The triple compounds, when purified, present some curious chemical phenomena, which a minute quantity of the double compound adhering to the crystals that I operated upon, prevented me from observing in the experiments I have already communicated to the Society. I shall describe these phenomena as they are produced by the triple compound of potassium, as this substance is most easily procured in considerable quantities, but as far as I have been able to observe the phenomena presented by the compound of sodium, are precisely analogous.

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* [From the Phil. Trans. for 1814. Read before the Royal Society, June 16, 1814.]

The triple compound of potassium, purified by alcohol, is almost tasteless, has no action on vegetable colours, is very little soluble in cold water, but more soluble in hot water; when it is thrown into concentrated nitric, or sulphuric, or phosphoric acids, it has no violent action on them. By heat it may be dissolved in them; and the solutions, when saturated, congeal and form crystalline substances intensely acid. When the substance formed by the triple compound and the nitric acid is strongly heated, the nitric acid flies off, and at the temperature at which it is entirely expelled, the substance itself begins to decompose, and affords a little iodine and much oxygen.

If the solution of the triple compounds in sulphuric or phosphoric acids, be heated strongly at the temperature at which the acids sublime, the triple compound itself is decomposed, and it affords oxygen and iodine, and leaves acid sulphate and phosphate of potassa. If when the mixture is rendered fluid by heat, a little sugar or other combustible matter is added, there is a violent action, and iodine is disengaged with great rapidity.

The triple compound dissolves without decomposition in solution of phosphorous acid; but on heating the solution, oxygen is attracted by it, iodine appears, and phosphate of potassa is formed.

When the triple compound is thrown into concentrated muriatic acid, there is an effervescence, the smell of chlorine is perceived, the fluid becomes yellow, and when evaporated, yields the chlorionic acid.

When the solution of the hydroionic acid in water is poured upon the triple salt, iodine is instantly produced in great quantities.

Acetic and oxalic acids dissolve the triple compound without decomposing it. On heating the solution in oxalic acid, the acid becomes brown from the deposition of charcoal, and iodine immediately appears.

When the triple compound is thrown into solution of sulphurous acid, iodine is instantly produced, and sulphuric acid formed, and if the sulphurous acid is not in too large a proportion, the solution becomes yellow by dissolving iodine; if more sulphurous acid is added, water is decomposed, and sulphuric acid and hydroionic acid formed.

The *double compound* of potassium and iodine has no action on oxalic, acetic, sulphurous, or phosphorous acids, but when it is mixed with the triple compound, it is instantly decomposed by them, and iodine set free.

The same double compound in its pure state is decomposed very slowly by muriatic acid; and to convert the greater portion into muriate of potassa (potassane), it is necessary that the acid should be very frequently distilled from it, and a part always remains unaltered; when mixtures of the triple and double compounds are exposed to the action of muriatic acid, potassane (muriate of potassa) is instantly formed; and if the proper proportions are adopted, none of the double or triple compounds remain, and the results are potassane only, and the oxychloric acid.

Mixtures of the triple and double compounds produce abundance of iodine when acted on by glacial hydrophosphoric acid gas, but the pure double compound affords only hydroionic acid gas, and this decomposition offers the best method which has yet occurred to me of procuring pure hydroionic acid. When the two substances are gently heated together, the hydroionic acid gas, which comes over in considerable quantities, forms a colourless solution when absorbed by water.

I have endeavoured to ascertain the composition of the triple compound of potassium. Seven grains that had been dried at the temperature of boiling water, heated to redness in a small crucible of platinum, lost 2·2 grains. Seven grains heated to dull redness in a small tube of glass, lost 1·7 grain: a minute portion of iodine, condensed in the middle part of the tube, but no violet vapour was observed in the upper part of it, and there was a very slight appearance only of moisture, so that the loss of weight in this last experiment must be principally ascribed to the expulsion of oxygen.

On a comparison of the results of these two analyses, it appears very probable that this triple compound is composed of one proportion of iodine about 165, one of potassium, 75, and six of oxygen, 90; which is a composition analogous to that of the hyperoxymuriate of potassa. The quantities that I used in my experiments were too small to render these results more than approximations; yet the similarity of them to those presented by the hyperoxymuriates ought, perhaps, to render them more worthy of confidence.

I have attempted to obtain pure triple compounds from solutions of baryta and lime, and from magnesia diffused through water, by dissolving iodine in them by heat, and by evaporating the clear liquor until it began to deposit crystals. In this way I have procured substances, which when well washed in distilled water, afforded no iodine to nitric acid, which yielded chlorine and chlorionic acid when acted upon by muriatic acid, and which, when distilled, afforded much oxygen and some iodine, and left substances which appeared to be mixtures of the earths with compounds that afforded iodine to sulphuric acid, producing a smell of sulphurous acid gas; and which probably consisted of the metals of the earths united to iodine.

The triple compounds from lime and magnesia were soluble without affording iodine in sulphuric acid; but on evaporating the acid, at the time that the vessel of platinum in which the experiment was made became dry and almost red-hot, the violet vapour was perceived. Even the triple compound from baryta did not afford iodine or oxygen by treatment with sulphuric acid, except under the same circumstances.

3. When I first discovered that the triple compounds dissolved in acids without effervescence, I thought it probable that the effect depended upon the formation of a compound of oxygen and iodine, similar to euchlorine, or the oxychloric acid, and which remained dissolved in the fluid; and on this idea I made a number of experiments with the hope of obtaining such a combination in a detached form.

I distilled the solution of the triple compound of potassium in sulphuric acid, but the only gaseous product I obtained was oxygen. Sulphuric acid and iodine condensed in the cool part of the apparatus, and the residuum was acid sulphate of potassa.

Conceiving that a compound of oxygen and iodine might nevertheless exist in the fluid, and be decomposable at a high temperature, I attempted to obtain it by acting on the triple compound of barium by sulphuric acid, and by evaporating the fluid obtained at a gentle heat, and suffering it to cool at different periods of the process; but in this manner of operating I gained no better results.

The triple compound of barium is scarcely soluble in water. Water that had been boiled upon it gave only a slight cloudiness to sulphuric acid, which possibly might be owing to some double compound mixed with it; the fluid, when evaporated nearly to dryness, afforded fumes which had the characters of those of sulphuric acid, and by a red heat yielded iodine, and left sulphate of baryta.

When the solid triple compound of baryta was heated in very small quantities of diluted sulphuric acid, the fluid separated exhibited acid properties, and when gradually evaporated left a substance which congealed by cooling, and formed a solid of a yellow colour deliquescent in the air, strongly acid, and which reddened vegetable blues, and did not afterwards destroy them. When strongly heated, the substance afforded the same results as the substance procured from the fluid just mentioned.

The residual solid matter obtained by the action of sulphuric acid on the triple compound of barium was treated a second time with sulphuric acid, yet notwithstanding, when heated to redness, it yielded iodine in abundance.

I have repeated these experiments very often, because M. Gay Lussac has stated that an acid compound of oxygen and iodine may be procured by dropping sulphuric acid into a solution of the triple compound of barium; but the conclusions of this ingenious chemist seem to have been founded upon the want of effervescence in the process; and his experiments were made at a very early period of the investigation, and probably before this time he has found reason to alter his opinion.*

It is probable that a binary compound of iodine and oxygen may be formed, but the facts presented by the action of acids on the triple compounds are not sufficient to prove its existence.

When small quantities of very dilute sulphuric acid are digested on the triple compounds of potassium and barium, the fluid obtained is always acid, and always precipitates muriate of baryta. I thought it possible that the compound of iodine and oxygen might possess this property; but on collecting the precipitate and examining it, it appeared to be a mixture of the triple compound and sulphate of baryta, and from all the facts it appears that in the action of acids on the triple compounds new combinations only are formed.†

I take the liberty of proposing for the triple compounds the names of oxy-iodes, because when decomposed by heat, they afford oxygen and iodine. Individually they may be named from their bases. Thus *oxypotassame*, or oxy-iode of potassium, will signify the triple compound of potassium, oxygen, and iodine, and *oxybarame*, or oxy-iode of barium, will denote the triple compound of barium.*

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* [Vide the author's subsequent paper on a compound of iodine and oxygen for further explanation.]

† When sulphuric acid is made to dissolve as much of the triple compound of potassium by heat as possible, the mixture congeals by cooling into a yellow transparent substance, extremely deliquescent and very acid. On decomposing it by heat, *neutral* sulphate of potassa remains. Now as oxygen and iodine are the only substances driven off by heat, it may be asserted that the acid property of the mixture depends upon these two principles; yet this conclusion does not follow according to sound chemical logic: iodine alone destroys the alkaline properties of potassa, and oxygen and iodine in combination with potassium form a difficultly soluble and almost tasteless substance. This substance, the triple compound, has only a weak attraction for sulphuric acid, and it might be expected that in combining with sulphuric acid it would not deprive it of its acid properties.

II. *Some Observations on Hydroionic Acid, and on the Compound procured by means of it.*

1. I have generally procured the hydroionic acid which I have used in my experiments by the process referred to in the last section, the action of hydrophosphoric acid on potassame, but I have sometimes employed the gas procured from moistened iodine by phosphorus.

The hydroionic acid gas is rapidly decomposed by being heated in contact with oxygen, and a solution of iodine and hydroionic gas in water is formed, and it is slowly decomposed by heat alone, affording a deep red brown easily fusible substance, which seems to be a compound of hydroionic gas and iodine.

When condensed in water, it is instantly decomposed by solution of nitric acid and iodine precipitated.

The solution of hydroionic acid rapidly absorbs oxygen from the air, and becomes yellow, and at last deep tawny orange; and this absorption is assisted by light and heat, the hydrogen is attracted by the oxygen to form water, and the iodine formed is dissolved in the remaining acid.

The concentrated hydroionic acid will probably form a good eudiometrical substance; it does not render the vessels in which it is used cloudy like the hydro-sul phurets by the deposition of solid matter, and it does not enlarge the volume of the residual air like some other substances.

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* [They are now commonly called Iodates.]

The solution of the hydroionic acid is decomposed by being heated with the hyper-oxymuriate of potassa, and iodine is produced.

Hydroionic acid gas, as I have mentioned in my last paper, is decomposed by all the metals I have exposed it to, except gold and platinum; and the same metals that decompose it in its gaseous state, likewise decompose it when it is in solution, requiring, however, in some cases, the assistance of heat. The fluid hydroionic acid tarnishes silver at common temperatures, and dissolves mercury slowly when boiled in contact with it.

2. It dissolves the alkaline and common earths, and forms with them compounds very analogous in their properties to the compounds they produce when acted on by muriatic acid.

I heated dry quick lime in a small tube filled with hydroionic acid gas, a yellow fluid immediately formed, which was coloured by dissolving hydroionic gas and iodine, and a fusible compound soluble in water, and which had a bitter taste similar to muriate of lime, was produced.

I made the same compound by dissolving marble in the hydroionic acid; the compound when heated to redness became fluid, and when kept in fusion in contact with air emitted iodine, gradually lost its fusibility, and from being neutral became alkaline, so that at a high temperature iodine is partly expelled from calcium by oxygen. I proved this still more distinctly by fusing the compound in a close vessel, in which it was confined by mercury. There was no change. I admitted a little oxy-potassame, and caused it to give off oxygen by heating it: as soon as the calcareous compound was fused in contact with oxygen, it instantly emitted iodine, and lime was formed on the surface.

The compound formed from hydroionic acid and baryta is an acrid bitter substance, very similar in its taste to *barytane* (fused muriate of baryta), not decomposable when heated to whiteness unless oxygen is present, but when it is heated in contact with oxygen, oxygen is absorbed, and a part of its iodine expelled.

Magnesia dissolved in hydroionic acid without effervescence, and the solution evaporated gave a solid substance, having a taste very similar to muriate of magnesia. Like that salt, it partly lost its acid by a red heat; but a portion remained not decomposable out of the contact of air, but which instantly afforded iodine when heated in contact with oxygen.

I dissolved glucina, ittria, and zircona in the hydroionic acid; they formed neutral saline compounds. The compound of hydroionic acid and glucina was less soluble, and more astringent in taste than the muriate of glucina, and was entirely decomposed when heated in the open air, affording hydroionic acid and iodine.

The compound formed from ittria was more soluble, and highly astringent; that formed from zircona astringent, with more of bitterness. Both these salts were decomposed when heated in the atmosphere at a low red heat; a smell of hydroionic gas was perceived, iodine was produced, and the earths remained.

3. I mentioned, in a note, Montpellier, Jan. 10, (containing a correction for my last communication to the society,) that the alkaline property which I at first supposed to belong to the compounds of potassium and sodium with iodine, depended upon some undecomposed subcarbonate of potassa mixed with the hydrate

of potassa I employed, as the subcarbonate of potassa is decomposed by iodine and carbonic acid set free, I had not thought it probable that the subcarbonate of potassa could interfere with the result. But I find that if the subcarbonate exist at all in the lixivium, a portion of it always remains undecomposed. I find likewise, that when a solution of iodine in lixivium of potassa is rendered perfectly neutral, or even slightly acid by hydroionic acid, a strong red heat renders the solid substance obtained slightly alkaline, provided it be in contact with air. Whether the separation of iodine by oxygen, in this instance, depends upon some effect of the moisture contained in the atmosphere, or upon the continued action of fresh portions of oxygen on the same surface of the compound, it is not easy to say; but a similar effect I find is produced upon *potassile* (fluate of potassa); this substance gains the power of reddening paper tinged with turmeric, by being strongly heated in contact with the air.

The power of neutralizing acids does not belong to the true compound of iodine and potassium, but depends either on the subcarbonate not decomposed, or upon the alkali formed during the ignition of the compound; the pure double compound seems to have no power of action on the acids it does not decompose; I fused it in contact with sulphurous acid gas confined by mercury in a glass tube, the salt gained a slight tint of yellow, but did not absorb its own volume of gas; after this, it slightly reddened litmus, so that the acid must have had little more than a mechanical adhesion to the salt.

When potassane, or iode of potassium, is fused with boracic acid, there is a perfect mixture of the two bodies. In my first researches on this mixture, I conceived that they entered into chemical union, and formed a violet-coloured glass, and that the acid property of the boracic acid was neutralized by the new compound; but I since find that the violet colour of the glass is owing to the developement of iodine, and when the application of heat is long continued, much iodine is disengaged, and the colour of the glass changes to olive, and borate of potassa is formed. When the glass is dissolved in warm water, an olive coloured powder separates, soluble when boiled in the caustic alkalies, so that there is great reason to suppose that it is boron, and that the boracic acid is decomposed by the attraction of the potassium combined with the iodine for oxygen, assisted by the tendency of iodine to assume the elastic state.

I fused the neutral compound of iodine with silica; no change was effected when the experiment was made in close vessels, but when the mixture was exposed to air, and intensely heated, a little iodine was evolved, some potassa formed, and some silica dissolved by it.

III. *On other Acid Compounds of Iodine.*

1. I have made several experiments on the combination of iodine and chlorine, obtained by admitting chlorine in excess to known quantities of iodine in vessels exhausted of air, and repeatedly heating the sublimate. Operating in this way, I find that iodine absorbs less than one-third of its weight of chlorine.

The compound of iodine and chlorine is a very volatile substance, and in consequence of its action upon mercury, I have not been able to determine the elastic force of its vapour. Hence the estimations of its composition from experiments on the quantity of chlorine absorbed in close vessels must necessarily be liable to error. In one experiment in which I dissolved the sublimate, by admitting a small quantity of water into the retort, I found that eight grains of iodine had caused the disappearance of five and a quarter cubical inches of chlorine.

In another experiment in which the sublimate was not dissolved by water, and in which the absorption was judged of by the admission of fresh quantities of the gas, twenty grains caused the disappearance of 9·6 cubical inches of chlorine, the barometer being at 30·1, and the thermometer at 57° Fahrenheit.

It seems probable, from these experiments, that the chlorionic acid consists of one proportion of iodine, and one of chlorine.

The chlorionic acid formed by the sublimation of iodine in chlorine in great excess is of a bright yellow colour, when fused it becomes of a deep orange, and when rendered elastic it forms a deep orange-coloured gas. It is capable of combining with much iodine when they are heated together, its colour becomes in consequence deeper, and the chlorionic acid and the iodine rise together in the elastic state.

The solution of the chlorionic acid in water likewise dissolves large quantities of iodine, so that it is possible to obtain a fluid containing very different proportions of iodine and chlorine.

The pure solution of the chlorionic acid, when it is very diluted, loses its colour by being agitated for some time in contact with chlorine, and then, when poured into fixed alkaline lixivia or solutions of the alkaline earths, it causes the precipitation of substances having the characters of triple compounds or the oxyiodes. If it is coloured, or in its ordinary state, at the same time that the oxyiode is precipitated, much iodine appears, and it is impossible to render a *concentrated* solution colourless by agitation with chlorine, or to deprive it of its power of yielding iodine by acting on alkaline solutions. The chlorionic acid, when poured into a solution of muriate of baryta, causes a copious precipitate in it, which has all the characters of oxyiode of barium, and the liquor becomes very acid.

When the colourless solution of chlorionic acid is added to a strong solution of ammonia, a white powder is precipitated which detonates feebly by a gentle heat; and which, when decomposed in glass vessels, affords iodine and elastic matter which does not support combustion.

When highly coloured chlorionic acid is employed, the powder that falls down is black, and detonates with much greater force, and by the slightest touch or motion, and it appears to be the same substance as that procured directly by the action of iodine on ammonia, and which I have shewn to be a compound of azote and iodine. Whether the white powder is a similar substance containing a larger proportion of azote, or whether it is a compound of ammonia with oxygen and iodine, or with iodine and chlorine, I have not yet been able to determine.

It is soluble in excess of chlorionic acid, and in this way may be separated from the black powder; it affords a little moisture during its detonation, but it is not possible to say whether this is formed in the process, or whether it is water adhering to the compound, for the temperature of its decomposition is so low, that a proper degree of heat cannot be applied to render it dry.

When two bodies so similar in their characters and in the compounds they form, as iodine and chlorine, act upon substances at the same time, it is difficult to form a judgment of the different parts that they play in the new chemical arrangements produced. When I found that the chlorionic compound formed a strong acid by solution in water, I at first suspected that water was decomposed, and hydroionic acid and euchlorine formed; there was no effervescence in the process, and the proportions agree with the supposition; but I find that solution of euchlorine instantly decomposes hydroionic acid and precipitates iodine, which is afterwards redissolved by the chlorine set free; and nitric acid, which decomposes hydroionic acid, has no action on chlorionic acid.

It was possible, likewise, that if water was decomposed, muriatic acid and a compound of iodine and oxygen might exist in the solution; I endeavoured to ascertain if this was the case by distilling the solution at different temperatures, and collecting the products, but I obtained always the same fluid.

When coloured solution of chlorionic acid is boiled with hyper-oxymuriate of potassa, it loses its colour, and chlorine is given off from it; but in this case it likewise gradually loses its acidity, and a substance which yields iodine by heat with much effervescence, and which is probably oxypotassame, is precipitated.

It appears to me most probable that the acid property of the chlorionic compound depends upon the combination of the two bodies; and its action upon solutions of the alkalies and the earths may be easily explained, when it is considered that chlorine has a greater tendency than iodine to form double compounds with the metals, and that iodine has a greater tendency than chlorine to form triple compounds with oxygen and the metals.

When in the case of the action of the chlorionic compound in fixed alkaline lixivia, the chlorine is not in great excess, much iodine is always set free, because as it is easy to perceive from the proportions in which they combine, there is not sufficient oxygen detached from the alkali by chlorine to form the triple compound; and if the estimation of the composition of oxypotassame given in the first section, be accurate, supposing that none of the double compound of iodine is formed, a solution must contain five proportions of chlorine to one of iodine, to produce a triple compound without the precipitation of iodine. It is, however, most probable that some double compound of iodine is always formed, as a solution must be extremely diluted indeed to contain five proportions of chlorine to one of iodine.

When the solution of chlorionic acid is poured into solution of muriate of baryta, water must be decomposed to furnish hydrogen to the muriatic acid, and oxygen to the triple compound, and in this case some double compound of iodine and barium must be formed, and remain dissolved in the solution.

From the action of chlorionic acid on metallic solutions, I am inclined to believe that triple compounds of the common metals, oxygen and iodine may be formed by means of it. It occasions a copious precipitate without effervescence in the solution of sulphate of iron, and in the solutions of nitro-muriate of lead, and tin, and of nitrate of copper, and from analogy it is probable that these precipitates consist of the metal, oxygen and iodine.

2. I thought it probable from the rapid action of tin on iodine, that tinfoil would burn in the vapour of iodine, but on introducing it into the violet-coloured gas in a small retort, made very hot, though the combination was instantaneous, yet no light was apparent.

I thought it possible that the acid properties of the compound of tin and iodine, which I have described in my last communication to the Society, might depend upon the decomposition of water, and upon the formation of hydroionic acid. On this idea I distilled the solution of it in water, hoping, if hydroionic acid were formed, that I should obtain some in this process; but the fluid that came over was merely water coloured by a minute quantity of iodine, and the orange coloured substance which remained when dissolved in water, exhibited the same acid properties as before, and combined with ammonia without affording any oxide.

The compound of iodine and iron when dissolved in water exhibited acid properties, but when the solution was distilled it yielded hydroionic acid, and deposited oxide of iron, and the entire solution acted on by ammonia, afforded an olive coloured precipitate in great abundance.

IV. *On the Action of some Compound Gases on Iodine.*

1. I heated some iodine in a dry glass globe filled with sulphuretted hydrogen; there was a considerable absorption of gas, no sulphur was deposited, and a reddish brown fluid was formed, which when thrown into water rendered it strongly acid, and deposited much sulphur; the water passed through a filter exhibited the properties of hydroionic acid.

It is evident fron this experiment, that sulphur, iodine, and hydrogen, are capable of forming a triple compound.

2. I sublimed some iodine in dry olefiant gas; a little of a reddish brown fluid was formed, but the greatest part of the iodine crystallized on the sides of the vessel

in which the experiment was made. By repeating the process several times, more of the fluid was formed. It was volatile at a moderate heat, and gave a yellow tint to water, but did not render it acid; there was a very slight absorption of the gas.*

3. Iodine submitted to nitrous gas effected no change in it.

4. When iodine was exposed to carbonic oxide, it underwent no change; it was repeatedly sublimed in it in the common daylight without undergoing the slightest alteration.

When the violet gas was formed by heating iodine in carbonic oxide, and the vessel exposed for some time to direct solar rays, a small quantity of a limpid fluid which had an acrid taste formed in the vessel. An accident prevented me from ascertaining if any gas had been absorbed; but it seems probable from this result that, like chlorine, iodine may be combined with carbonic oxide by the agency of light.

V. *On the mode of Detecting Iodine in Combinations, and on certain Properties of its Compounds with Sodium.*

1. I have examined many of the marine productions of the Mediterranean, with the view of determining whether they contained iodine. The ashes of the fuci and ulvæ of this sea afford it in much smaller quantities than the *sel de varec,* and in a very few cases only have I been able to obtain evidences of its existence in them.

2. M. Berard was so good as to order a considerable

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* [A solid compound of these two substances has since been obtained by exposing them together to the sun's rays.]

quantity of the species of ulva, which abounds on the coast of Languedoc, to be burnt for me in his laboratory at Montpellier. The ashes consisted, for the most part, of common salt, but a small quantity of alkaline lixivium, which was obtained from them, afforded a red fluid when acted upon by sulphuric acid, and a similar colour, I found, was produced, when a solution of subcarbonate of soda and common salt, containing a minute quantity of the compound of sodium and iodine, was treated in the same manner by the acid.

3. One of the best tests of the presence of a very minute quantity of iodine in compounds, is their action upon silver. Water, when it contains less than $\frac{1}{1000}$ part of its weight of the double or triple alkaline compounds of iodine, tarnishes polished silver.

The effect produced by compounds of iodine, may be distinguished from that produced by sulphurets or sulphuretted hydrogen by this circumstance, that solutions containing sulphurets or sulphuretted hydrogen, by being boiled with a little muriatic acid, no longer tarnish the metal; whereas the solutions containing iodine still retain the power.

4. Amongst the number of sea-weeds that were obligingly given me for examination, by Professor Viviani, of Genoa, the ashes of the following afforded slight indications of the presence of iodine:

Fucus cartilagineus.
——— membranaceus.
——— rubens.
Fucus filamentosus.
Ulva pavonia.
—— linza.

In the ashes of the corallines and sponges, I could discover no evidences of the presence of the substance.

5. I have examined three specimens of alkali formed by the combustion of vegetables that grow on the sea shore, one from Sicily, one from Spain, and the third

from the Roman States; but not one of them afforded any indications of the presence of iodine.

6. I evaporated a considerable quantity of sea-water procured at Sestri of Levanto, in Liguria, in a part of the bay remote from any source of fresh water; but I could gain no unequivocal evidences of the presence of the compounds of iodine in it. The residual liquor, after the common salt had been separated, did not act upon silver, nor colour sulphuric acid. The first crystals of salt which fell down when fused upon silver, appeared to me to tarnish it more than the last; from which it appeared probable that they may have contained some triple compound of iodine; yet, after being ignited, they did not colour sulphuric acid. When a large quantity of this water was electrized by a Voltaic apparatus, and the products separated at the positive pole collected in a small cup of gold, which was covered with cement, except in the interior and lower part forming the circuit, a yellow solution was obtained, which, when it was exposed to the negative pole of a Voltaic apparatus, yielded a black powder fixed in the fire, and not unlike the compound formed by heating gold and iodine together; but the quantity was too minute to admit of analysis, and a dark coloured substance is likewise obtained by negatively electrifying oxymuriate of gold, and there can be no doubt but that this substance formed a principal part of the solution.*

If iodine exists in sea-water, which there is every reason to believe must be the case, though in extremely minute quantities, it is probably in triple union with oxygen and sodium, and in this case it must separate with the first crystals of common salt.

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* Iodine, like chlorine, I find, combines with gold and platinum when heated with them, or when they are exposed to them in its nascent state. [Traces of iodine have since been detected in the sponges and coarse salt of the Mediterraneum, vide Vol. I. p. 179.]

Whether the superiority which curers of fish and meat are in the habit of attributing to bay salt over rock salt, is at all connected with the presence of the compounds of iodine, is an inquiry perhaps worth making, and the results of Dr. Henry's elaborate investigation of the composition of different kinds of salts, do not preclude the possibility of the circumstance, though they certainly diminish the probability.

I rubbed pieces of beef that had been killed some days with the double and triple compounds of sodium. They did not putrefy; the one rubbed with the double compound became very tender and soft, and of a red-brown colour; that exposed to the triple compound hardened considerably, and became of a paler colour.

The triple compound, as I have mentioned before, has very little taste, and neither of the compounds seems to have any pernicious quality when received into the stomach. I fed a gold-finch with bread soaked in water, holding in solution the double compound for a few days, and he drank water, holding in solution the triple compound for three days, without apparently suffering any inconvenience.

Florence, March, 23, 1814.

XVIII.

SOME EXPERIMENTS ON THE COMBUSTION OF THE DIAMOND AND OTHER CARBONACEOUS SUBSTANCES.*

SINCE it has been shewn by various accurate experiments, that the diamond and common carbonaceous substances consume nearly the same quantity of oxygen in combustion, and produce a gas having the same obvious qualities, a number of conjectures have been formed to explain the remarkable differences in the sensible qualities of those bodies, by supposing some minute difference in their chemical composition; these conjectures have been often discussed, it will not be necessary therefore to dwell upon them. MM. Biot and Arago, from the high refractive power of the diamond, have supposed that it may contain hydrogen; I ventured to suggest in my third Bakerian Lecture, from the circumstance of its non-conducting power, and from the action of potassium upon it, that a minute portion of oxygen may exist in it; and in my account of some new experiments on the fluoric compounds, I hazarded the idea that it might be the carbonaceous principle combined with some new light and subtile element of the class of supporters of combustion. M. Guyton de Morveau, who conceived he had proved by experiments made fourteen years ago, that common carbonaceous substances were oxides of diamond, from his last researches, conducted in the same manner as those of Messrs. Allen and Pepys, seems still inclined to adopt this opinion, though in admitting a much smaller quantity of oxygen than he originally supposed in charcoal; and he considers the diamond as pure carbonaceous matter, containing, possibly, some atoms of water of crystallization.

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* [From Phil. Trans. for 1814. Read before the Royal Society, June 23, 1814.]

I have long had a desire of making some new experiments on the combustion of the diamond and other carbonaceous substances, and this desire was increased by the new facts ascertained with respect to iodine, which, by uniting to hydrogen, affords an acid so analogous to muriatic acid, that it was for some time confounded with that substance. My object in these experiments, was to examine minutely whether any peculiar matter was separated from the diamond during its combustion, and to determine whether the gas, formed in this process, was precisely the same in its chemical nature, as that formed in the combustion of common charcoal. I have lately been able to accomplish my wishes. I shall now have the honour of communicating my results to the Royal Society.

During a stay that I made at Florence in the end of March and beginning of April,* I made several experiments on the combustion of the diamond, and of plumbago, by means of the great lens in the Cabinet of Natural History; the same instrument as that employed in the first trials on the action of the solar heat on the diamond, instituted by Cosmo III. Grand Duke of Tuscany; and I have since made a series of researches on the combustion of different kinds of charcoal at Rome, in the laboratory of the Academia Lyncei.

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* [Vide Vol. I. Col. Works, p. 181.]

In the first series I was honoured by the assistance of the Count Bardi, the Director, and Signior Gazzari, the Professor of Chemistry at the Florentine Museum, and in the last, by that of Signiors Morrichini and Barlocci, Professors of the College Sapienza at Rome.

In the very first trial on the combustion of the diamond, I ascertained a circumstance that I believe has not been noticed before, namely, that the diamond, when strongly ignited by the lens in a thin capsule of platinum perforated with many orifices, so as to admit a free circulation of air, continues to burn in oxygen gas after being withdrawn from the focus. The light it affords is steady, and of so brilliant a red, as to be visible in the brightest sunshine, and the heat produced is so great, that in one experiment, in which three fragments of diamonds weighing 1·84 grain only were burnt, a fine wire of platinum, used for attaching them to the tray, was fused, and that some time after the diamonds were removed out of the focus.

The knowledge of this circumstance enabled me to adopt a very simple apparatus and mode of operation in my researches, and to complete, in a few minutes, experiments which have been supposed to require the presence of bright sunshine for many hours.

My apparatus consisted of clear glass globes of the capacity of from fourteen to forty cubical inches, having single apertures to which stop-cocks were attached; a small hollow cylinder of platinum, which I used in experiments with the blow-pipe, was attached to one of the stop-cocks, and was mounted with a little perforated capsule of platinum for containing the diamond. When the experiment was to be made, the globe containing the capsule and the substance to be burned was exhausted by an excellent air-pump, and pure oxygen gas, made from hyperoxymuriate of potassa, admitted. The globe before and after the experiment was brought to the same temperature as the water over which the oxygen gas had remained. And as during the short time required for the combustion there was no sensible change either in the thermometer or barometer, no corrections for pressure or temperature were rendered necessary; the change of volume in the gas after the combustion, was estimated by means of a fine tube connected with the stop-cock, adapted by a proper screw to the stop-cock of the globe, and the absorption was judged of by the quantity of mercury that entered the tube, which afforded a measure so exact, that no alteration, however minute, could be overlooked. As the elastic force of the vapour of water is the same at the same temperature, it was evident, that if any water formed in these experiments, it would be deposited as dew or mist in the globe; and I am convinced by direct trials, that a quantity of moisture not capable of being weighed by a balance sensible to the $\frac{1}{100}$ of a grain, is rendered evident by deposition on a polished glass surface.*

The diamonds were always heated to redness before they were introduced into the capsule.

During the combustion of the diamond, the glass globe was kept cool by the application of water to that part of it immediately above the capsule, and where the heat was greatest.

In the first experiment, three diamonds, weighing together 1·63 grain, were entirely consumed, in a quantity of oxygen gas, more than three times as much as was necessary to convert them into carbonic acid. In this case, after the combustion had once commenced, it continued without a fresh application of the lens till there remained only a very thin piece of the largest diamond in contact with the capsule, and this being brought into the focus, rapidly disappeared. On restoring the globe to its original temperature, there was a very evident deposition of moisture; but on arranging the apparatus, so as to ascertain the change of volume of the gas, there entered only twenty-one grains of mercury. In this experiment the cylinder of platinum had been fastened into the stop-cock by means of a small perforated cork, it seemed probable, when the small diminution of gas was considered, that the appearance of moisture might be owing to the production of vapour from this cork during the combustion, and the second experiment demonstrated that this was the case.

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* A piece of paper weighing a grain was introduced into a tube of about the capacity of 4 cubical inches, the exterior of which was gently heated by a candle; immediately a slight dew was perceptible in the interior of the upper part of the tube; the paper taken out and weighed immediately in the balance above referred to, had not suffered any appreciable diminution.

In this second experiment, 1·84 grain of small diamonds were employed, and a glass globe of the capacity of 14·9 cubical inches. Soon after the capsule was placed in the focus in bright sunshine, the diamonds burnt with great brilliancy, and continued to burn till they had considerably diminished in bulk; but their splendour of combustion gradually became less, and before they had apparently lost half of their volume the process ceased. By placing them a second time in the focus, after agitating the globe so as to change their places, the combustion was again produced; but the light was much less vivid than before, and the combustion continued a much shorter time. They were exposed to the concentrated rays a third and a fourth time, but after the fourth time they seemed incapable of burning, and though kept for some minutes in the focus, appeared to undergo no farther diminution: two fragments remained, which, as it was afterwards found, weighed ·52 of a grain: the barometer during the experiment was at 29·9 inches, the thermometer at 56° Fahrenheit. When the original temperature of the globe was restored, there was not the slightest appearance of vapour or humidity; the interior was as clear as before the experiment, and there was no solid matter of any kind separated in the tray. The fragments of diamond which remained were not black, but had lost their lustre like glass that had been acted on by fluoric acid, nor at any period of the process was any carbonaceous appearance perceived upon them. When the communication was made by the stop-cock between the interior of the globe and a surface of mercury, a minute quantity entered equal to 1·5 grain only.

A portion of the gas in the globe was transferred into a tube in the mercurial apparatus, and the oxygen it contained absorbed by the combustion of phosphorus; 3·5 parts of gas heated in this way left a residuum of 2·5 parts. A portion of the gas was agitated with lime water, when seven parts out of ten were absorbed. I exposed the gas which remained after the combustion of phosphorus to several tests; it had not only the obvious characters of carbonic acid, but exhibited exactly the same chemical phenomena. Potassium, strongly heated in it in a small glass tube over mercury, burnt with a dull red light, and formed an alkaline product of the same intense black colour as that produced by its combustion in the carbonic acid procured by the dissolution of marble: distilled water absorbed rather less than its own volume of the gas, and became subacid, sparkled by agitation, gained the taste and smell of a solution of carbonic acid in water, precipitated in the same manner lime water, and when in excess redissolved the precipitate. To ascertain if this precipitate was exactly the same in composition as pure carbonate of lime, I made a sufficient quantity of it, by pouring lime-water into the recipient containing the results of the first experiment; and after collecting and drying it at the temperature of 212° Fahrenheit, I introduced a quantity of it contained in some foil of platinum through mercury into a glass tube filled with mercury, and I heated in the same manner an equal quantity of finely powdered Carrara marble, and admitted to them equal quantities of solution of muriatic acid. In this trial, there was rather more elastic fluid disengaged from the Carrara marble than from the carbonate of lime from the diamond; but on examining the foil of platinum after the experiment, I found that a little of the carbonate had not been acted upon: I tried two similar experiments, substituting bibulous paper instead of the metallic foil for infolding the carbonates; the results were such as to shew that both substances afforded the same quantities of elastic fluid.

I heated some of the carbonate from the diamond in a tube, which contained potassium, and passed the potassium through it in vapour: there was ignition, and a substance of a dense black colour was formed; this substance was acted on by dilute muriatic acid, when it left a fine black powder, which burnt like lampblack, and when thrown into fused nitre, scintillated and disappeared in the same manner as powdered charcoal.

The gas that remained in the second experiment, after the absorption of the carbonic acid gas, vividly supported combustion, and diminished with nitrous gas, but as the degree of purity of the oxygen gas with which the globe was filled had not been determined before the experiment, it was impossible to ascertain with precision that no elastic matter had been emitted during the process. To determine this point, I made a third experiment: a thin diamond, weighing ·93 of a grain, was introduced into the platinum capsule, which was placed in a globe filled with water and inverted in water, some oxygen gas, the last portion from the decomposition of hyperoxymuriate of potassa, was thrown into the globe, so as to displace the water below the level of the capsule. The focus of the great lens was thrown upon the capsule, which with the diamond was immediately rendered dry by it, and the diamond soon entered into combustion and burnt as usual. After the process was finished, the carbonic acid was absorbed by lime water, and the remaining gas, which equalled about one-third of the quantity of oxygen originally used, was compared analytically in several experiments with a portion of the same oxygen as that introduced into the globe; two measures of nitrous gas being added to a measure of each of the gases, the diminution was less by from $\frac{1}{100}$ to $\frac{2}{100}$ parts in the cases in which the gas that had been exposed to the action of the diamond was used; but this minute difference is what might have been expected, and which indeed could not fail to exist, when it is considered that, during the absorption of carbonic acid gas by water and lime-water, a small quantity of common air is always expelled from the water.

In this last experiment a small fragment of diamond remained unconsumed, which was similar in appearance to that mentioned in the second experiment, and its colour, which was originally yellow, was rendered rather darker. In no one of the three experiments was there any distinct appearance of carbonization, when the process was stopped in consequence of the impurity of the gas; though the diamonds were of various colours and different lustres.

A piece of plumbago from Borrowdale in Cumberland, weighing two grains, was exposed in the focus in the same manner as the diamond in the first and second experiments, having been previously heated red; the quantity of oxygen gas employed was 8·5 cubical inches: more than half the plumbago remained unconsumed, and during the combustion some ashes were produced. The phenomena in this experiment were very different from those observed in the experiments on the diamond, the gas became clouded during the process, and there was a considerable deposition of dew on the interior of the globe. When the original temperature of the globe was restored, and the stop-cock opened, 96·6 grains of mercury entered, and drops of moisture even were observed condensed on the sides of the vessel.

In the second series of experiments, charcoal formed by the action of sulphuric acid on oil of turpentine, and some produced during the formation of sulphuric ether, from which nitric acid had been distilled, and which afterwards had been strongly ignited, and charcoal of oak which had undergone the same process, were used.

Three grains of charcoal from turpentine were employed, 2·5 of that from alcohol, and five grains of the charcoal of the oak: in all these instances of combustion, the gas became clouded during the combustion; and when the original temperature of the globe was restored, moisture was found condensed in the interior, much the largest quantity in the experiment on the charcoal of oak, and the least in that on the charcoal procured from oil of turpentine. The charcoal from oak left a residuum of white ashes, which was principally carbonate of lime; that from oil of turpentine produced no residuum; that from alcohol, which was formed in a common process of the manufacture of ether, left a minute quantity of ashes, probably owing to the impurity of the sulphuric acid employed.

The quantity of mercury which entered the apparatus, indicating the change of volume of the gas, was, in the experiments—

On the charcoal of oil of turpentine .	107·5 grains.
In that on the charcoal of alcohol .	194·5
In that on the charcoal of oak . .	513·3

From the results of these different experiments, it appears evident, that the diamond affords no other substance by its combustion than pure carbonic acid gas; and that the process is merely a solution of diamond in oxygen, without any change in the volume of the gas; for the slight absorption in the second experiment is scarcely more than a compensation for the volume occupied by the diamonds consumed.

It is likewise evident that in the combustion of the different kinds of charcoal, water is produced; and from the diminution of the volume of the gas, there is every reason to believe, that the water is formed by the combustion of hydrogen existing in the charcoal, and experiments which I have referred to, or detailed in my third Bakerian Lecture, prove the presence of hydrogen in common charcoal; and as the charcoal from the oil of turpentine left no residuum, no other cause but the presence of hydrogen can be assigned for the diminution occasioned in the volume of the gas during its combustion.

M. Guyton de Morveau* has noticed the production of water during the combustion of plumbago from Keswick, and from these experiments it is most probable, that it is formed in the process of combustion, for it is unlikely that water should remain in union with plumbago at a red heat; and in the various experiments that I have made on the ignition of plumbago by Voltaic electricity, I have never perceived the separation of any moisture, or the production of any gas; so that it seems most likely that it contains intimately combined hydrogen. It cannot be supposed that water exists in it in union with oxide of iron, for in this case, there would be no obvious cause for the diminution of the volume of the gas; and all analogy is in favour of the conclusion that the iron is in a metallic state.

The general tenour of the results of these experiments is opposed to the opinion, that common carbonaceous substances differ from the diamond by containing oxygen; for in this case they ought to increase and not diminish the volume of the oxygen; nor on the other hand, is it favourable to the supposition, that the diamond contains oxygen, for the difference in the quantity of carbonic acid produced in the different experiments, is no more than may be reasonably ascribed to the generation of water in the combustion of the common carbonaceous substances; and the results of the experiments to which I have referred in the beginning of this paper, on the action of potassium on the diamond, may be easily accounted for from other circumstances.†

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* Annales de Chimie, tom. lxxxiv. p. 241.

† See Bakerian Lecture for 1808. Potassium decomposes the silica in glass, by being heated in contact with it, and in the case in which equal quantities of potassium were long heated in glass tubes, the one in contact with diamonds, the other alone, that in contact with the diamonds must have acted upon a much greater surface of glass.

That charcoal is more inflammable than the diamond may be explained from the looseness of its texture, and from the hydrogen it contains; but the diamond appears to burn in oxygen with as much facility as plumbago, so that at least one distinction supposed to exist between the diamond and common carbonaceous substances is done away by these researches.

A fact which I formerly noticed, the blackening of diamond by the long continued action of heated potassium, induced me to suspect in the beginning of these inquiries, that common charcoal might owe its colour, opacity, and conducting power, to the circumstance of its containing minute portions of the metals of the alkalies, or earths, and plumbago to the iron it contains; but when I found that charcoal made from oil of turpentine, which left no residuum in burning, and charcoal precipitated from carburetted hydrogen gas by chlorine, had the same properties, it was necessary to renounce the opinion.

The only chemical difference perceptible between diamond and the purest charcoal, is that the last contains a minute proportion of hydrogen; but can a quantity of an element, less in some cases than $\frac{1}{50000}$ part of the weight of the substance, occasion so great a difference in physical and chemical characters? This is possible, yet it is contrary to analogy, and I am more inclined to adopt the opinion of Mr. Tennant, that the difference depends upon crystallization. Transparent solid bodies are in general non-conductors of electricity, and it is probable that the same corpuscular arrangements which give to matter the power of transmitting and polarizing light, are likewise connected with its relations to electricity; and water, the hydrates of the alkalies, and a number of other bodies which are conductors of electricity when fluid, become non-conductors in their crystallized form.

The power possessed by certain carbonaceous substances of absorbing gases, and separating colouring matters from fluids, is probably mechanical and dependent upon their porous nature; for it belongs in the highest degree to vegetable and animal charcoal, and it does not exist in plumbago, coke, or anthracite.

The same conclusions respecting the composition of carbonic acid may be drawn from these experiments, as from those of Messrs. Allen and Pepys, and Theodore de Saussure. If the calculations be founded upon the difference of the weights of oxygen and carbonic acid gases, which appears the most exact method, carbonic acid gas will contain, according to the estimate of the mean specific gravities of the two gases, given by Theodore de Saussure,* thirty parts of oxygen, or two definite proportions, to 11·3 parts of carbon, or one definite proportion.

Supposing the diminution of the oxygen produced in the experiments on the common carbonaceous substances entirely occasioned by the formation of water, it is easy to calculate the proportions of hydrogen in them; but in the case of plumbago there is probably a diminution of oxygen, from the oxidation of the iron; and it is not certain that the ashes afforded by the charcoal from vegetable substances exist in it in the state of earths and alkalies; and as the quantity of hydrogen varies according to the degree of heat to which charcoal has been exposed, it is almost useless to attempt to assign its proportion in any particular case, especially when the largest is extremely minute.

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* Annales de Chimie, tom. lxxi. p. 261. This estimation is the same as that I have given, Elements of Chem. Phil. page 223.

The nature of the chemical difference between the diamond and other carbonaceous substances may be demonstrated by another process, namely, igniting them in chlorine; when common well-burnt charcoal, or plumbago from Cumberland, is intensely ignited in chlorine, white fumes are immediately perceived in consequence of the production of muriatic acid gas by the hydrogen, which acid precipitates the aqueous vapour in the chlorine; but the diamond occasions no such effect. A small diamond, weighing ·45 of a grain, was kept in a state of intense ignition by the great lens of the Florentine Museum for more than half an hour, but the gas suffered no change, and the diamond had undergone no diminution of weight, and was not altered in appearance.

Charcoal, after being intensely ignited in chlorine, is not altered in its conducting power, or colour; and this circumstance is in favour of the opinion, that the minute quantity of hydrogen is not the cause of the great difference between the physical properties of the diamond and charcoal.

XIX.

SOME EXPERIMENTS ON A SOLID COMPOUND OF IODINE AND OXYGEN, AND ON ITS CHEMICAL AGENCIES.*

IN the two papers containing researches on iodine which the Royal Society has done me the honour of publishing in the Transactions, I have described a class of bodies consisting of iodine, oxygen, and different bases analogous to the hyperoxymuriates. In the last of these papers, I mentioned, that I had not been able to procure any binary combination of iodine and oxygen from these compounds, neither by the method proposed by M. Gay Lussac, namely, the action of sulphuric acid on the oxyiode of barium, nor by other methods of my own institution; and that in experiments on the effects of the acids on the oxyiodes, new combinations only were formed. I have lately resumed this inquiry, and by pursuing a new and entirely different plan of operation, I have at last succeeded in combining oxygen and iodine. In the following pages I shall describe the circumstances which led me to ascertain the existence of this compound, and I shall detail some experiments on its analysis and its chemical agencies.

In the course of my researches, I observed, that when a solution of the compound of iodine and chlorine was poured into alkaline solutions, or even into certain muriatic solutions, the precipitate was an oxyiode; and this fact seemed to indicate, that iodine had a stronger attraction for oxygen than chlorine; iodine, likewise, has an attraction for chlorine; it appeared, therefore, extremely probable, that euchlorine, or the gaseous combination of oxygen and chlorine, would be decomposed by iodine, and two compounds formed, one of oxygen and iodine, and the other of iodine and chlorine, or that a triple compound would be produced from which chlorine could be easily separated, and on submitting the idea to the test of experiment, I found that I had not been deceived.

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* [From the Phil. Trans. for 1815. Read before the Royal Society, April 20, 1815.]

To produce the compound of oxygen and iodine, it is necessary merely to bring the euchlorine and iodine together at the ordinary temperature of the atmosphere. As soon as the euchlorine comes in contact with the iodine, there is an immediate action, its colour changes to bright orange, and a liquid is formed. When the euchlorine is in sufficient quantity, a white substance likewise appears. By the application of a gentle heat, the orange compound of chlorine and iodine may be made to rise in vapour; and the compound of oxygen and iodine remains.

When this compound is required to be dry, the euchlorine should be passed through dry muriate of lime (calcane) before it is admitted to the iodine. The apparatus that I have employed for producing the substance is a curved bent tube, in the form of an inverted L (ᒣ), closed at one end, the closed leg of the tube being longest, and which serves as a retort for generating the gas; a thin long-necked glass receiver for containing the iodine, and a curved tube of smaller diameter than the first, and cemented or ground into it for conveying the gas into the receiver. The muriate of lime is placed in some dry paper in the upper part of the large curved tube; and to produce the substance from 40 grains of iodine, 100 grains of the hyperoxymuriate should be used, and four times the quantity of solution of muriatic acid of specific gravity about 1·105; a very small spirit lamp should be employed to generate the gas; and to prevent explosions, the heat should be applied with great care, and only to the bottom of the tube.

The compound of oxygen and iodine, when entirely freed by heat from the compound of oxygen and chlorine, appears as a white semi-transparent solid; it has no smell, but a strong astringent sour taste. Its specific gravity is considerable, for it rapidly sinks in sulphuric acid. When heated strongly, it decomposes, undergoing fusion at the moment, and is entirely converted into gaseous matter and iodine, leaving no residuum whatever.

It requires for its entire decomposition, a heat which is rather below the boiling point of olive oil, and there seems to be little or no increase of temperature in the process.

Its nature is proved both by analysis and synthesis, for when euchlorine acts upon iodine, the volatile substance produced has all the characters of the body produced by the immediate action of chlorine upon iodine; and when the compound I am describing is decomposed in a pneumatic apparatus, the gas formed is found to be pure oxygen, and the solid sublimate produced is pure iodine.

I endeavoured to determine the proportions of the elements in the compound, by decomposing it in glass tubes carefully weighed, and ascertaining the loss of weight of the tube, and the volume of oxygen evolved. I have used very small quantities of the substance, but as my balance is delicate, I do not think there can be any considerable error in the results. I give those which I consider as the most accurate.

In one experiment, 3 grains of the substance afforded a quantity of oxygen equal to 517·3 grain measures of water, and lost in weight ·68. In a second experiment, 2 grains afforded 348·3 grain measures of oxygen. In a third experiment, 1 grain yielded 191 grain measures of oxygen.

Many experiments that I have lately made, have convinced me, that in my first paper I rated the number representing the proportion in which iodine combines too low; indeed at the time, I stated that my results afforded rude approximations, they demonstrated merely that iodine was represented by a very high number. In an experiment recently made with care, 50 grains of the iode of potassium decomposed by nitric acid, afforded 32·8 grains of nitre. According to this result, the number representing the proportion in which iodine combines is 227·3; but I do not venture to state this number as exact, as I am not secure of the purity of the hydrate of potassa from which the iode was made.

The compound of iodine and oxygen is very soluble in water; it slowly deliquesces in a moist atmosphere, but remains unaltered when the air is dry; its solution first reddens, and then destroys vegetable blues, and reduces other vegetable colours to a dull yellow. When its aqueous solution is heated, as the water rises in vapour, it gradually thickens, gains the consistence of syrup, becomes pasty, and at length by a stronger heat yields the solid substance unaltered; unless a sufficient heat is applied to decompose a portion of it, when it gains a purplish tint apparently from some iodine set free. The pasty substance that it forms is evidently an hydrate, for it yields moisture during its decomposition.

Its action upon inflammable bodies is such as might be expected from its composition. When it is heated, mixed with charcoal, sulphur, resin, sugar, or the combustible metals in a finely divided state, detonations are produced; and its solution rapidly corrodes all the metals which I have exposed it to, and it acts both upon gold and platinum, but much more intensely on the first of these metals.

When a solution of it is poured into solutions of the alkalies or alkaline earths, or when made to act on their carbonates, oxyiodes, or triple compounds of oxygen, iodine, and the metallic bases, are the results. By its action on solution of ammonia, a substance is produced apparently the same as that which is formed by the action of the compound of iodine and chlorine, saturated with chlorine on the same solution, and which I have mentioned in a former paper, and which, consequently, must be regarded as an oxyiode of ammonia.

When an aqueous solution of the compound is poured into a solution of the soluble salts of baryta and strontia, a copious precipitate of their respective oxyiodes is produced. The oxyiode of barium, as I have mentioned in my last communication on iodine, is a compound very slightly soluble in water, that of strontium is rather more soluble; and those of calcium, magnesium, glucinum, ittrium, aluminum, zirconum, are more so, and I believe in the order in which they have been named.

It forms combinations with all the metallic oxides I have tried its agency upon, and precipitates lead and mercury from their nitrous solutions.

The action of the compound upon acids is much more singular than that upon alkalies, earths, or metallic oxides. It appears to form combinations with all the fluid or solid acids to which I have been able to expose it, that it does not decompose. When sulphuric acid is dropped into a concentrated solution of it in hot water, a solid substance is precipitated, which consists of the acid and the compound; for on evaporating the solution by a gentle heat, nothing rises but water. On increasing the heat in an experiment of this kind, the solid substance formed, fused; and on cooling the mixture, rhomboidal crystals formed of a pale yellow colour, which were very fusible, and which did not change at the heat at which the compound of oxygen and iodine decomposes, but sublimed unaltered. When urged by a much stronger heat, it partially sublimed, and partially decompsed, affording oxygen, iodine, and sulphuric acid.

With hydro-phosphoric acid, the compound presents phenomena precisely similar, and they form together a solid, yellow, crystalline combination.

It dissolves in solution of hydro-phosphorous acid, but on heating the combination, iodine is immediately produced by a decomposition of part of the compound, and the remaining part unites to the phosphoric acid formed.

When hydro-nitric acid is poured into a concentrated solution of it, white crystals form in plates of a rhomboidal figure, and which when dried, partially decompose, and partially sublime at a much lower heat than the sulphuric or phosphoric compounds, and afford hydro-nitric acid, oxygen, and iodine.

It dissolves in solution of oxalic acid, but by a very gentle heat, the oxygen of the compound acts on the inflammable bases of the acids, and iodine and carbonic acid are disengaged in great quantities.

By liquid muriatic acid, the substance is immediately decomposed, and the compound of chlorine and iodine is formed.

When boracic acid was added to a solution of the new compound, it dissolved in it by heat, and did not crystallize on cooling. By evaporation, a solid white substance was procured, not so easily decomposed by heat as the compound itself.

The taste of all these acid compounds is very sour, though in different degrees of intensity: they redden vegetable blues, and they dissolve gold and platinum. When they are made to act on the alkalies or earths, or on saline solutions which they are capable of decomposing, common neutral salts and oxyiodes are formed at the same time.

The facts of the combination of the new compound with acids, serve to explain the phenomena of the action of these substances on the oxyiodes which I have described in my last paper on iodine, and they confirm the opinions there stated on the nature of this action. The substance procured by M. Gay Lussac, by the action of sulphuric acid on the solution of the oxyiode of barium, and which he has supposed to be a pure combination of oxygen and iodine mixed with a little sulphuric acid, has evidently for its base the combination just now described of sulphuric acid and the new compound, and, as I have shown, it likewise contains baryta. However minute the quantity of sulphuric acid made to act on oxyiode of barium, a part of it is always employed to form the compound acid; and the residual fluid contains both the compound acid, and a certain quantity of the original salt.

That this compound acid is a true chemical combination, is evident from the observations already detailed, and from its crystalline form. There is every reason to believe, that the proportions of its elements are definite. In one experiment I found, that a small quantity of the new compound in being converted into the rhomboidal crystals, gained rather less than half its original weight from the addition of the acid, i. e. 2 grains became 2·8 grains.

In experiments in which the products of the decomposition of the compounds from phosphoric and sulphuric acids were collected, the acids disengaged were found in their state of hydrates, from which it is probable, that the crystalline compounds are hydrates, and that the common acids carry their definite proportion of water into the combination. It is not indeed unlikely, that the presence of water is connected with the phenomenon of combination, and there is an instance of this kind which I long ago pointed out. Sulphurous acid gas, and nitrous acid gas, appear to have no action on each other, unless water be present; but with the vapour of water they form a solid crystalline hydrate.

Reasoning from analogy, it is probable, that a compound of oxygen may be formed, containing less oxygen than the new compound. I have made many experiments with the hope of discovering a body of this kind; but without any decided success.* When the solution of the new compound is made to act on the double compound of iodine and the alkaline metals, iodine is produced, which during its sublimation, yields no gaseous product. Iodine heated in a solution of the new compound slightly colours it, but this appears to be merely in consequence of its combining with the water; and the iodine rises in vapour with the water without decomposing the compound. In some experiments on the action of euchlorine on iodine, in which the iodine was in great excess, the solid substance formed had a chocolate tint; but this may possibly have depended upon a small quantity of free iodine, and when dissolved in water, it afforded, by the evaporation of the water, the white compound only.

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* [The existence of this compound is still problematical; according to M. Sementini, when the vapour of iodine and oxygen gas are heated together, such a one is formed: his results require confirmation: vide Quarterly Journal of Science, N. S. vol. i. 478.]

I detailed in my last paper on iodine, some unsuccessful attempts to procure a compound of oxygen and iodine from the chlorionic acid, the substance produced by the agency of the combination of iodine and chlorine in water, on the idea that water was decomposed in this experiment. I have made some further researches, on the supposition that it might contain a compound of iodine containing less oxygen than this new substance; but without any success: neither by distillation at very low temperatures, nor by the action of small quantities of oxide of silver, nor by any other means, have I been able to separate any compound of oxygen from it; and when it forms triple compounds, the oxyiodes, by its action upon alkalies, or earths, or metallic solutions, it appears that the oxygen of the alkalies or earths is only newly combined at the moment of its operation upon them, an effect assisted by the attraction of the bases of the earths for chlorine. The conclusion which I formed, that the chlorionic acid is a simple combination of the chlorionic sublimate in water, is still farther proved by the circumstances of the action of muriatic acid on the new solid compound of oxygen and iodine.* Page 498.

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* The chlorionic acid offers an easy method of procuring pure baryta. By dropping a solution of it into solution of muriate of barium, as I have shown in my last paper on iodine, a precipitate of oxyiode of barium is produced, which when decomposed by a strong heat, yields pure baryta,

As I have called the compounds of oxygen, iodine, and bases, *oxyiodes*, I venture to propose a name in conformity, that of *oxyiodine* for the new solid compound, and *oxiodic* acid, for the acid compound it forms with water. M. Gay Lussac, as I am informed, has proposed in a paper which I have not yet been so fortunate as to procure, but which is said to contain many new and important facts, the name of iodic acid for the compound of oxygen and iodine, the existence of which he conceived he had proved by his experiment on the action of sulphuric acid on the oxyiode of barium, and the terms iodats for the substances, consisting of oxygen, iodine, and bases. I am willing to pay every compliment to the sagacity of this ingenious chemist, in anticipating the knowledge of the nature of a body, the separate existence of which he had not demonstrated by experiment; but the term *iodic* acid does not appear to me sufficiently definite. For the hydroionic acid, and the chlorionic acid, as well as the oxyiodic acid, may be all called as a class iodic acids, or acids from iodine, and the termination in *at*, would place the oxyiodes in the common class of neutral salts, from which they differ in many respects. When they become binary compounds in consequence of their decomposition by heat, though they lose all their oxygen, their neutral and saline character remains unaltered, which is not the case with any other known class of bodies, except the hyperoxymuriates; and the terms iodes and oxyiodes which I proposed in the first paper, in which the distinction between these bodies was pointed out, sufficiently express the nature of the double and triple compound, and the difference between them.

the attraction of oxygen for barium being, as I have stated, stronger at this temperature than that of iodine.

I am desirous of marking the *acid* character of oxyiodine combined with water, without applying the name acid to the anhydrous solid. It is not at all improbable that the action of the hydrogen in the combined water is connected with the acid properties of the compound; for this acid may be regarded as a triple combination of iodine, hydrogen, and oxygen, an oxyiode of hydrogen, and it is possible that the hydrogen may act the same part in giving character, that potassium, sodium, or the metallic bases perform in the oxyiodes; and as hydrogen combined with iodine forms a very strong acid, and as this acid would remain, supposing all the oxygen to be taken away from the oxyiodic acid, it is a fair supposition that its elements must have an influence in producing the acidity of the substance.

Rome, February 10, 1815.

XX.

ON THE ACTION OF ACIDS ON THE SALTS USUALLY CALLED HYPEROXYMURIATES, AND ON THE GASES PRODUCED FROM THEM.*

THE effects produced when concentrated hydro-sulphuric acid (oil of vitriol) is poured upon hyper-oxymuriate of potassa, have been often objects of chemical discussion; the acid and the salt, it is well known, become deep orange, and if any moisture is present, or if heat be applied to the mixture, a detonation occurs. In a paper read before the Royal Society, I have ventured to suppose, that the phenomena depend upon the development and sudden decomposition of the compound of chlorine and oxygen, which I have named euchlorine.

A statement, which I understand has been made by M. Gay Lussac, namely, that a peculiar acid, which he has called chloric acid, may be procured from the hyper-oxymuriate of baryta by sulphuric acid, led me to examine the action of acids on the hyper-oxymuriates under new circumstances, and I have made some observations which appear to me not unworthy of being communicated to the Royal Society.

If 30 or 40 parts of sulphuric acid be poured upon one part of dry hyperoxymuriate of potassa in a wine glass, and the salt be agitated in the acid, there is a very slight effervescence only, the acid becomes of a deep orange tint, and white fumes, mixed with orange fumes, fill the upper part of the glass, which have a very peculiar and not a disagreeable smell.

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* [From the Phil. Trans. for 1815. Read before the Royal Society, May 4, 1815.]

The *slight* effervescence taking place in this process, induced me to suppose that the substance which coloured the acid must contain a larger proportion of oxygen than euchlorine, for I have shown, in a work published in 1812,* that hyperoxymuriate of potassa contains six proportions of oxygen; and by its decomposition 2·5 volumes of oxygen ought to be evolved for every volume of chlorine; and euchlorine procured from the hyperoxymuriate of potassa by solution of muriatic acid, yields only one volume of oxygen, and two volumes of chlorine.

I endeavoured to procure the substance which coloured the sulphuric acid during its action upon hyperoxymuriate of potassa, and after several failures, in which explosions took place, I at length succeeded in the following manner. Dry oxymuriate of potassa in powder was mixed with a small quantity of sulphuric acid, and they were rubbed together with a spatula of platinum till they had incorporated, and formed a solid mass of bright orange-colour. This mass was introduced into a small retort of glass, and exposed to the heat of water which was gradually warmed; a bright yellowish green elastic fluid arose from the mixture, which was rapidly absorbed by water, giving to it its own tint, but which had no sensible action on mercury.

To make this experiment without danger, not more than 50 or 60 grains of the hyperoxymuriate should be employed, great care should be taken to prevent any combustible matter from being present, and the water should not be permitted to attain a temperature equal to 212°, which may be easily managed by mixing it with alcohol. There are dense white fumes when the mixture is first made, but there seems to be no heat produced; a small quantity of the orange gas is disengaged at this time; but the greater part of it remains attached to the sulphuric acid in the solid mass, and is expelled from it by the heat.

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* Elem. of Chem. Phil. [Vol. IV.]

The gas procured by this process over mercury, when compared with the gas procured from the hyperoxymuriate, by liquid muriatic acid, is found to have a much more brilliant colour, is much more rapidly absorbed by water, has a peculiar and much more aromatic smell, unmixed with any smell of chlorine. It destroys moist vegetable blues without previously reddening them. When it is heated to a temperature about that of boiling water, it explodes with more violence than euchlorine, and greater expansion of volume, producing much light. After the explosion over mercury, rather less than three (from 2·7 to 2·9) volumes appear for two of the gas decomposed, and of these, two are oxygen, and the remainder chlorine.

A little chlorine is always absorbed by the mercury during the explosion of the gas; and it appears reasonable to conclude, that the deep yellow gas is in reality composed of two in volume of oxygen, and one of chlorine, condensed into the space of two volumes, and that it consists in weight, of one proportion of chlorine 67, and four of oxygen 60.

None of the combustible bodies which I have tried, decompose this gas at common temperatures, except phosphorus; this when introduced into it, occasions an explosion, and burns in the liberated gases with great brilliancy.

Its saturated solution in water is of a deep yellow colour, it does not taste sour, but is extremely astringent and corroding; when applied to the tongue, it leaves for a long while a very disagreeable sensation.

It occurred to me that the gas procured from the hyperoxymuriate by the action of liquid muriatic acid, might be a mere mixture of this gas and chlorine; and two in volume of this gas and three in volume of chlorine, would produce by explosion the same products as euchlorine. The only fact which I am acquainted with, opposed to the idea, is the circumstance of Dutch foil not burning spontaneously in the gas from muriatic acid, which might be expected if it contained as much as $\frac{3}{5}$ of uncombined chlorine; though the force of this argument is suspended, till it is supported by an experiment showing that Dutch foil inflames in a mixture of two of the deep yellow gas, and three of chlorine. I have not yet been able to procure at Rome, metallic foil fitted for this experiment.*

I have ascertained that the gas from hyperoxymuriate and muriatic acid, though it acts much more slowly upon water than the other gas, yet in the end gives it the same tint and properties; and when much of it is exposed to a small quantity of water, it always leaves a residuum of chlorine, so that if it be not a mixture, but a compound, the new gas is formed from it by the action of water.

The action of hydro-nitric acid on the hyperoxymuriate, affords the same gas as that produced by the action of sulphuric acid, and a much larger quantity of nitric acid may be safely made to act on the salt; but as the gas must be procured by solution of the salt, it is always mixed with about $\frac{1}{5}$ of oxygen. From the solid mixture made with sulphuric acid, I have obtained a gas containing only $\frac{1}{20}$ of oxygen; the fifth proportion obtained in the experiments with nitric acid, being evolved during the time the mixtures were made.

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* [This conclusion has been rather confirmed by the results of the experiments of Mr. Soubeiran, published in the 48th vol. of Ann. de Chimie et de Physique, and by some which I have obtained and ar inserted in the Edinburgh New Philosophical Journal, vol. xvii.]

The saturated solution of the gas affords white fumes, similar to those produced at the moment the hydrosulphuric mixture is made, from which it is probable, that these fumes consist of a hydrate of the gas.

The saturated solution, when mixed with solution of fixed alkalies, or of ammonia, does not immediately lose its colour, nor neutralise the alkalies; but after some time the effect is produced, and hyperoxymuriates are obtained, (probably mixed with a minute quantity of muriates). The solution exposed to air, or suffered to remain in close vessels, becomes soon colourless; and I am inclined to believe that this depends upon a decomposition of water, for some of it exposed to a small quantity of air rather increased its volume.

I shall not propose to give any name to this substance, till it is determined whether euchlorine is a mixture or a definite compound, and I hope soon to have the means of making a decisive experiment on this subject.

It appears that this new substance, though it contains four proportions of oxygen, is not an acid; and hence it is probable, that the acid fluid compound of oxygen, chlorine, and water, which M. Gay Lussac calls chloric acid, owes its acid powers to combined hydrogen, and that it is analogous to the other hyperoxymuriates, which are triple compounds of inflammable bases, chlorine, and oxygen, in which the base and the chlorine determine the character of the compound. Muriate of potassa (potassane) is a perfectly neutral body; and when six proportions of oxygen are added to it, it still remains neutral. Muriatic acid (chlorine and hydrogen) is a strong acid; and according to the relation above stated, it ought not to lose its acid powers by the addition of six proportions of oxygen. Till a pure combination of chlorine and oxygen is obtained, possessed of acid properties, we have no right to say that chlorine is capable of being acidified by oxygen, and that an acid compound exists in the hyperoxymuriates. We know that chlorine is capable of being converted into an acid by hydrogen, and, as I mentioned in my last paper, where this principle exists its energies ought not to be overlooked; and all the new facts confirm an opinion which I have more than once before submitted to the consideration of the Society, namely, that acidity does not depend upon any *peculiar* elementary substance, but upon *peculiar combinations* of various substances.

Rome, Feb. 15, 1815

Note.

Since my return to England, I have made some farther investigations on oxyiodine, on the oxyiodes, and on the deep yellow gas. The portable apparatus which I employed in Italy, enabled me to operate only on very minute quantities of oxyiodine; I have lately made my experiments on a larger scale.

Thirteen grains of oxyiodine decomposed by heat, afforded 9·25 cubical inches of oxygen: and 48 grains of oxypotassame or oxyiode of potassium, yielded when decomposed by heat, 31 cubical inches of oxygen gas: and 30 grains of potassame or iode of potassium (a portion of the salt so decomposed,) afforded by treatment with nitric acid 17·8 grains of dry nitre. These results give the number 246 as the number representing iodine, and prove that oxyiodine consists of one proportion of iodine and five of oxygen; and that the oxyiodes contain six proportions of oxygen.

The deep yellow gas when mixed with chlorine in the proportion of 2 to 3, or even of 2 to 2, deprives it of the power of acting upon Dutch foil, though 1 of chlorine when mixed with 2 of common air, still burns this substance. Hence it appears probable, that the deep-coloured gas and chlorine have a chemical action on each other, and that euchlorine is not a simple mixture of them. I hope soon to be able to present to the Society, some new results on this subject. [This argument seems to lose its force when it is considered, that euchlorine has a similar effect on chlorine, as I have found to be the case.]

London, June 12, 1815.

XXI.

ON THE ANALOGIES BETWEEN THE UNDECOMPOUNDED SUBSTANCES, AND ON THE CONSTITUTION OF ACIDS.*

IN a work published in 1812 (Elements of Chemical Philosophy), I have pointed out some of the analogies between the substances considered in the present state of our knowledge as undecompounded, and I have endeavoured to found a classification upon these analogies:—

I placed oxygen and chlorine together, because, in combining with inflammable bodies and metals, they produce heat and light in a much higher degree than any other known species of matter, and because many of their compounds are possessed of analogous chemical and electrical qualities. At the same time I stated that there is a general chain of resemblance between all the chemical agents; and that while sulphur is analogous to chlorine in one of its properties, it possesses more general resemblance to phosphorus.

The progress of chemical discovery since that time has added new links in the system of analogy, and modified some of the ancient links. The singular body, iodine, whilst it strongly resembles chlorine in most of its chemical qualities, is still more analogous than chlorine to sulphur; and in lustre, opacity, specific gravity, and the high proportional quantity in which it unites to other matter, it is similar to the metals. With the metals, indeed, it may be said to be distinctly connected by means of tellurium, which, as I have shown, by uniting to hydrogen, forms a substance having acid properties.

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* [From the Journal of Science and the Arts, edited at the Royal Institution, vol. i. (art. xviii.) 1816.]

Carbon, boron, and silicon, appear the links between phosphorus, and sulphur and the metals; and probably the bases of zircona, glucina, and alumina, will form a part of the chain between the metals of the alkaline earths and the common metals.

Hydrogen and azote stand almost alone; yet hydrogen is connected with the common inflammable bodies by the manner in which it combines with oxygen and chlorine, and azote resembles carbon in the proportional quantity in which it enters into combination, and in its want of attraction for metallic substances. Fluorine, probably, if it could be obtained insulated, would form the link between oxygen and chlorine and azote.

M. Gay Lussac, in an elaborate paper published in the "Annales de Chimie" for July 1814,* in which he advanced many views, reasonings, and calculations, upon the composition of the compounds of chlorine, exactly the same as those I have given in three papers published three years before in "The Transactions of the Royal Society,"* endeavours to show that there is a stronger analogy between chlorine, iodine, and sulphur, than between the same bodies and oxygen; and he wishes them to be separated as a class from oxygen, and placed in a class with sulphur. I do not admit the force of his reasoning on this subject; the bodies to which he refers bear only one marked point of resemblance to sulphur, that which I have mentioned above; and they differ from it in their electrical relations, and in the chemical and electrical nature of all their other compounds, and agree in these respects with oxygen. Like oxygen, in voltaic arrangements they are determined to the positive surface, whereas sulphur is separated at the negative surface. The compounds they form with metals strongly resemble those formed by oxygen: they are electric,

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* The historical notes attached to that paper are of a nature not to be passed over without animadversion. M. Gay Lussac states in these notes, that he and M. Thenard first advanced the hypothesis that chlorine was a simple body, and that he was the first person to demonstrate the nature of iodine; and he quotes M. Ampere as having had, before me, the opinion that chlorine and fluorine were simple bodies. On the subject of the originality of the idea of chlorine being an elementary body, I have always vindicated the claims of Scheele, but I must assume for myself, the labour of having demonstrated its properties and combinations, and of having explained the chemical phenomena it produces; and

I am in possession of a letter from M. Ampere, which shews that he has no claims of this kind to make.

With respect to the nature of the fluoric principle, still a hypothetical subject, M. Ampere was certainly original; but he formed his opinion in consequence of my views of chlorine; and I had imagined and applied the hypothesis before I had any communication from M. Ampere, and in my paper on the subject, I have done all the justice that was in my power to the views of that ingenious academician.

With regard to iodine, the first account I had of it was from M. Ampere, who, before I had seen the substance, supposed that it might contain a new supporter of combustion. Who had most share in developing the chemical history of that body, must be determined by a review of the papers that have been published upon it, and an examination of their respective dates. When M. Clement shewed iodine to me, he believed that the hydriodic acid was muriatic acid; and M. Gay Lussac, after his early experiments made originally with M. Clement, formed the same opinion, and *maintained it*, when I *first* stated to him my belief that it was a new and peculiar acid, and that iodine was a substance analogous in its chemical relations to chlorine.

* Phil. Trans. 1810, 1811.—[XVI. XVII. XIX. of this vol.]

and many of them soluble in water, and possessed of acid properties; whereas those formed by sulphur are all non-electric and insoluble.

I cannot admit M. Gay Lussac's views on the classification of the undecompounded substances, nor can I adopt his ideas respecting their properties as chemical agents. He considers hydrogen as an *alkalizing* principle, and azote as an *acidifying* principle. This is an attempt to introduce into chemistry a doctrine of occult qualities, and to refer to some mysterious and inexplicable energy what must depend upon a peculiar corpuscular arrangement. If hydrogen be an alkalizing principle, it is strange that it should form some of the strongest acids by uniting to bodies not in themselves acid; and if azote be an acidifying principle, it is equally strange that it should form nearly nine-tenths of the weight of the volatile alkali. It is impossible to infer what will be the qualities of a compound from the qualities of its constituents; and if M. Gay Lussac's views were correct, the prussic basis of azote and carbon ought to have its acid properties diminished, and not increased, as he has proved them to be, by combination with hydrogen.

When certain properties are found belonging to a compound, we have no right to attribute these properties to any of its elements to the exclusion of the rest, but they must be regarded as the result of combination.

When M. Gay Lussac assumes that oxygen and hydrogen, in the proportions in which they form water, are passive as elements of a combination, it is a *pure assumption*, and opposed to the whole series of chemical facts. Hydrogen with chlorine forms a strong acid; oxygen with phosphorus forms a strong acid; and supposing water combined with the compound of phosphorus and chlorine, the results contain two of the most energetic known acids: phosphorane does not redden litmus paper; but if it be dissolved in water, it becomes a solution of muriatic and phosphorous acids.

If oxygen and hydrogen, in the proportion in which they form water, are to be considered as passive, as neutralizing each other in all combinations in which they exist, then almost all the vegetable acids must be considered as acids of carbon, which, though containing much less oxygen than carbonic acid, and some of them less even than carbonic oxide, have yet strong acid powers.

I have discovered a gaseous combination of four proportions of oxygen and one of chlorine, which has no acid properties. M. Gay Lussac has discovered a compound of two proportions of hydrogen, one of chlorine, and six of oxygen, which has acid properties; but he considers this substance merely as chlorine acidified by oxygen, and neglects the hydrogen, without which he allows, however, it cannot exist. He supposes that this acid of one proportion of chlorine and five of oxygen exists in all the hyperoxymuriates, but he does not support his supposition by any proof. The hyperoxymuriates are, as I showed six years ago, composed of one proportion of chlorine, one of a basis, and six of oxygen. Hydrogen, in M. Gay Lussac's chloric acid, may be considered as acting the part of a base; and it is an important circumstance in the law of definite proportions, that when one metallic or inflammable basis combines with certain proportions of a compound, all the others combine with the same proportions.

M. Gay Lussac states, that if the chloric acid be not admitted as a pure combination of chlorine and oxygen, neither can the nitric or sulphuric acids be admitted as pure combinations of oxygen. This is perfectly obvious. An acid composed of five proportions of oxygen and one of nitrogen is altogether hypothetical; and it is a simple statement of facts to say that liquid nitric acid is a compound of two proportions of hydrogen, one of azote, and six of oxygen; and, as I showed long ago, the only difference between nitre and hyperoxymuriate of potash is, that one contains a proportion of azote, and the other a proportion of chlorine.

There are very few of the substances which have been always considered as neutral salts, that really contain the acids and the alkalies from which they are formed. The muriates and the fluates must be admitted to contain neither acids nor alkaline bases. Most of the prussiates, M. Gay Lussac has lately shown, are in the same case. Nitric and sulphuric acids cannot be procured from the nitrates and sulphates without the intervention of bodies containing hydrogen; and if nitrate of ammonia were to be judged of from the results of its decomposition, it must be regarded as a compound of water and nitrous oxide.

Only those acids which are compounds of oxygen and inflammable bases appear to enter into combination with the fixed alkalies and alkaline earths without alteration, and it is impossible to define the nature of the arrangement of the elements in their neutral compounds. The phosphate and carbonate of lime have much less the character attributed to neutro-saline bodies than calcane (muriate of lime), and yet this last body is not known to contain either acid or alkaline matter. The chloriodic acid, the phosganic acid, and the binary acids containing hydrogen, combine with ammonia without decomposition, but they appear to be decomposed in acting upon the fixed alkalies or alkaline earths; and yet the solid substances they form have all the characters which were formerly regarded as peculiar to neutral salts consisting of acids and alkalies, though they none of them contain the acid, and only the two first of the series the alkalies, from which they are formed.

The substitution of analogy for fact is the bane of chemical philosophy; the legitimate use of analogy is to connect facts together, and to guide to new experiments.

As I cannot adopt M. Gay Lussac's opinions, so neither can I approve of his nomenclature. To call the compounds of chlorine and iodine chlorures and iodures, is to place chlorine and iodine in the class of inflammable bodies, and I prefer to these denominations chlorides and iodes. M. Gay Lussac has called sulphuretted hydrogen hydro-sulphuric acid; a term which has already been applied to sulphuric acid, the oil of vitriol of commerce. Hydro-chloric acid would signify chloric acid combined with water, and therefore, according to M. Gay Lussac's own views, is more applicable to his chloric acid than to muriatic acid.

XXII.

ON THE PRUSSIC BASIS AND ACID.*

IN the last article I have defended some opinions of my own, and combated some of M. Gay Lussac's. In this article, the object I propose is much more agreeable to my feelings,—to offer my experimental confirmation of the very elaborate and ingenious researches of M. Gay Lussac on the prussic acid, and the prussic base.

The prussic acid (hydrocyanic acid) was procured by Mr. M. Faraday by M. Gay Lussac's process, and I found it of specific gravity rather below ·7. On electrizing this acid in the Voltaic circuit, by wires of platinum, it afforded oxygen at the positive surface, and hydrogen in about twelve times the volume at the negative surface. This at first led me to suspect the decomposition of azote: but on continuing the experiment for several hours, the production of oxygen ceased, and a compound of platinum and cyanogen was found at the positive pole, and hydrogen was given off at the negative pole; so that there was every reason to suppose that the oxygen arose from a minute portion of water, which could not be separated by muriate of lime.

By heating prussiate of mercury in muriatic acid gas, I obtained pure liquid prussic acid and corrosive sublimate.

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* From the Journal of Science and the Arts, vol. i. (art. xix.), 1816.

I burnt a large quantity of cyanogen, freed from prussic acid by red oxide of mercury, slowly in oxygen gas, and cooled the product by a freezing mixture, but no water was deposited.

I decomposed cyanogen by passing electrical sparks through it; when it gave its own volume of azote, and deposited charcoal.

I shall not detail any experiments of research on a subject which is peculiarly M. Gay Lussac's; but I shall venture to point out to him a mode which I have found successful of procuring combinations of cyanogen; that of heating bodies with prussiate of mercury. In this way, compounds of the prussic radicle with iodine, sulphur, and I believe with phosphorus, may be formed. The compound of iodine is a very curious body: it is volatile at a moderate heat, and on cooling collects in flocculi, adhering together like oxide of zinc formed by combustion, and it has a very acrid taste and pungent smell.

I wish M. Gay Lussac could be prevailed upon to give up the inexpressive and difficult names of cyanogen and hydrocyanic acid, and to adopt the simple ones of prussic gas and prussic acid.

XXIII.

REMARKS ON A NOTE IN THE SECOND NUMBER OF THE JOURNAL OF SCIENCE AND THE ARTS.*

In a paper printed in the second number of this Journal,† I have mentioned that M. Gay Lussac, in his first experiment on iodine made with M. Clement, supposed that it afforded muriatic acid. M. Gay Lussac, with whom I had the pleasure of conversing during a late visit that I made to Paris, informs me, that he did not assist M. Clement in those experiments, but that they were made in his laboratory.

When the analogy between hydroiodic and muriatic acids are considered, it is not difficult to conceive how they should have been at first confounded even by so skilful and accurate a chemist as M. Clement; and it is only in examining either their recondite properties, or their combinations, that they can be distinguished. Through the kindness of M. Ampere and M. Clement, I had made some experiments on iodine, before M. Gay Lussac had seen this substance, and assisted by my theory of the combinations of chlorine, I immediately formed the conclusion now generally adopted respecting its nature. I have never wished to arrogate to myself any merit in the history of the researches made respecting a discovery belonging to another country, and I take this opportunity of assuring M. Gay Lussac that the note which called forth the animadversions to which I replied, was more painful to me than it could have been to him. The merits of that school of chemistry, of which he is so distinguished an ornament, require no vindication. They are felt by the chemists of the present times; and they will convey its glory to remote posterity.

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* [From the Journal of Science and the Arts, vol. iii., 1817.]

† [The preceding article but one of this Volume.]

This subject of critical discussion carries me to another. M. Gay Lussac put into my hands, when I was at Paris, a work published some years ago on Eudiometry, by himself and M. Humboldt, which contains very curious observations on the effect of the dilution of mixtures of oxygen and hydrogen on explosion; had I been acquainted with these results I should have referred to them in my last paper on flame,* as they confirm my general conclusions: and MM. Humboldt and Gay Lussac have anticipated me in some of my objections to that theory, which supposes *condensation* the cause of those combinations resulting from increase of temperature.

I recommend the whole Essay as highly deserving perusal, and as a model of ingenuity and accuracy in physical research, worthy of its celebrated authors.†

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* [This paper will be given in the following vol.]

† [I have not thought it right to exclude from the Collected Works, this paper and the last but one which preceded it, as they relate to interesting and important points in the history of the progress of chemical science. Moreover, they appear to me well adapted to show how very averse their author was to controversy; the dislike he had to pass censure, and how he delighted rather in following his natural bias of bestowing praise, and of expressing admiration.]

XXIV.

ON AQUA-REGIA, OR NITRO-MURIATIC ACID.*

If strong nitrous acid, saturated with nitrous gas, be mixed with a saturated solution of muriatic acid gas, no other effect is produced than might be expected from the action of nitrous acid of the same strength on an equal quantity of water; and the mixed acid so formed has no power of action on gold or platina.

Again; if muriatic acid gas and nitrous gas in equal volumes be mixed together over mercury, and half a volume of oxygen be added, the immediate condensation will be no more than might be expected from the formation of nitrous acid gas. And when this is decomposed, or absorbed by the mercury, the muriatic acid gas is found unaltered, mixed with a certain portion of nitrous gas.

It appears then, that *nitrous* acid and muriatic acid gas have no chemical action on each other.

If *colourless nitric acid* and muriatic acid of commerce be mixed together, the mixture immediately becomes yellow, and gains the power of dissolving gold and platinum. If it be gently heated, pure chlorine arises from it and the colour becomes deeper; if the heat be longer continued, chlorine still rises, but mixed with nitrous acid gas, which is much more absorbable by water, and which may be separated from the chlorine by a small quantity of water. When the process has been very long continued till the colour becomes very deep, no more chlorine can be procured from it. It loses its power of acting upon gold and platinum, and nothing rises from it but a mixture of nitrous acid and muriatic acid.

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* [From the Journal of Science and the Arts, edited at the Royal Institution, vol. i. 1816.]

It appears then, from these observations, which have been very often repeated, that nitro-muriatic acid owes its peculiar properties to a mutual decomposition of the nitric and muriatic acid, and that water, chlorine, and nitrous acid gas are the results: and the attractions which produce these results, appear to be the attraction of oxygen for hydrogen to form water, and that of nitrous acid gas for water.

The correctness of these conclusions is still further proved by the circumstance, that though nitrous gas and chlorine have no action upon each other when perfectly dry; yet if water be present, there is an immediate decomposition, and nitrous acid and muriatic acid are formed.

It is easy to calculate the quantity of chlorine produced in nitro-muriatic acid by the doctrine of definite proportions. For every 101 parts in weight of nitric acid, equivalent to 118 parts in weight of hydro-nitric acid decomposed, 67 parts of chlorine must be produced.

The knowledge of the nature of aqua-regia will explain many peculiarities of its action upon metals and alkalies. It does not oxidate gold and platina, but merely causes their combination with chlorine; and when it produces neutral salts, they are mixtures, and not chemical combinations of nitrates and compounds of chlorine.

M. Berthollet, with his usual sagacity, long ago stated that nitro-muriatic acid owed its peculiar properties to the production of chlorine;* and, in substituting the theory that it is a simple, for the hypothesis of its being a compound body, his conclusions will be found in perfect harmony with those I have drawn.

M. Berzelius,† in a letter written in a tone wholly unworthy of a chemist of so exalted a reputation, has asserted that azotane, or the detonating compound of chlorine and azote, is *dry nitro-muriatic acid.* It is difficult to discover what meaning he attaches to this term; and it is wholly unnecessary to refute so unfounded and vague an assertion.

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* Chem. Stat. English Translation, vol. ii. p. 179.

† Thomson's Annals, vol. vi. p. 47.

XXV.

ON THE FALLACY OF THE EXPERIMENT, IN WHICH WATER IS SAID TO HAVE BEEN FORMED BY THE DECOMPOSITION OF CHLORINE.*

SOME experiments have been lately communicated to the Royal Society of Edinburgh, from which it has been inferred, that water is formed during the action of muriatic acid gas on certain metals, and consequently, that chlorine is decomposed in this operation.

In repeating these experiments, I have ascertained that the water is derived from sources not suspected by the authors, and that their conclusions are unfounded. To take up the time of the society by long experimental details and theoretical speculations on such an occasion, will be unnecessary; I shall therefore only transiently mention the sources of error, and demonstrate their operation by two or three examples.

When muriatic acid gas is passed through flint glass tubes heated to redness, a small quantity of water is formed by the action of the gas on the oxide of lead in the glass, and a smaller quantity by its action on the alkali of the glass, the process being one of double affinity, the hydrogen of the muriatic acid unites to the oxygen of the oxide, and the chlorine combines with the metals.

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* [From the Phil. Trans. for 1818. Read before the Royal Society, February 12th, 1818.]

A copious dew was formed by passing muriatic acid gas through flint glass tubes red hot, and a less copious dew, by passing it through green glass tubes. In the first instance, the glass became opaque, and gained a pearly lustre, and a combination of chlorine and lead sublimed from the hotter into the colder part of the tube. In the second, the surface of the tube became slightly opaque, but no sublimate was formed.

When fine clean iron wire was introduced into such tubes, and made red hot, and muriatic acid gas passed over it, no particular precautions being taken to free the tubes from common air, much more water appeared; but this excess of water principally owed its existence to the combination of hydrogen disengaged from the muriatic acid gas by the iron with the oxygen of the common air. I say, *principally*, because an inappreciable quantity must have been deposited from the vapour of hydrated muriatic acid in the muriatic acid gas. This was proved by filling the whole apparatus with hydrogen in another experiment, and generating the muriatic acid gas in a retort filled with hydrogen, when the water produced was no more than might have been expected from the action of the muriatic acid gas on the oxide of lead and alkali in the glass. I give the details. Above 21 grains of the first combination of chlorine and iron* were formed; the quantity of moisture collected by bibulous paper, and which was a strong acid solution of the proto-muriate of iron, amounted to less than half a grain, and of this not more than two-thirds could have been water. Now, if chlorine had been decomposed in this operation, the quantity of water ought to have been at least ten times as great.

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* [The protochloride of iron, fusible and not volatile.]

I have shewn by numerous experiments, that in the action of muriatic acid gas upon metals, hydrogen, equal in bulk to half the volume of the gas, is produced; it is therefore evident, that if water had been generated by the action of muriatic acid gas on metals, it must have been the *chlorine*, or the *metal*, or both, that were decomposed. As chlorine can be freed from much of its aqueous vapour by dry muriate of lime, which is not the case with muriatic acid gas, it offers a much more unexceptionable substance for experiments of this kind. I passed 23 cubical inches of chlorine slowly through dry muriate of lime into a flint glass tube red hot, containing a green glass tube full of iron wire; the chlorine combined with this iron with intense heat; the bright sublimate formed was passed through more iron wire heated to redness, so as to form a considerable quantity of the first compound of chlorine with iron, which, when examined, was found exactly the same as that produced by the action of muriatic acid gas on iron. All the products were heated strongly, and the end of the glass tube kept very cool; but *not the slightest appearance of moisture was perceptible.*

In all these experiments I was assisted by Mr. Faraday of the Royal Institution.

Muriate of ammonia is not altered by being passed through porcelain or glass tubes heated to redness, but if metals be present, it offers similar results to muriatic acid gas. In one experiment in which muriate of ammonia recently sublimed was used, instead of muriatic acid gas, the appearance of moisture was less than in the experiments on muriatic acid gas, which have been just detailed, and yet there was a considerable action on the oxide of lead in the glass, not only by the muriatic acid, but likewise by the free hydrogen of the decomposed ammonia.*

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* [This paper on the author's part closed the discussion relative to the nature of chlorine,—namely, that it is to be considered as a simple substance, because proof is wanting that it has ever yet been decompounded. It was in conducting this discussion, and that respecting the nature of potassium and sodium, and of potash 'and soda, that he established the capital principle of doctrine in chemical philosophy now referred to,—defining rigorously what is a chemical element,—cautiously designating it, as the attained limit of research,—not the attainable, and much less the absolute limit, which present knowledge, of course, cannot predicate. I perceive from the Report of the address delivered by the Rev. Vernon Harcourt, at the meeting of the Scientific Association over which he presided at Birmingham, that he lays emphasis on, and specially directs attention to this principle of logic, in tracing the advance of modern science, assigning the merit of its broad and impressive enunciation solely to the author,—a principle, it is to be hoped, that will always henceforward be held fundamental,—and to a certain extent give stability to chemical theory. The author himself gave the merit of its adoption to Lavoisier;—who certainly was the first to employ it; it was by its use, aided by appeal to statical results, that he succeeded in overthrowing the doctrine of phlogiston.]

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ERRATA.

Page 79, line 30, *for* ascension *read* accension.
113, note, line 15, *for* quality *read* quantity.
129, line 5, *omit* 1 before hydrogen.
233, — 29, *for* more *read* mere.
254, — 20, *for* phenomena *read* phenomenon.
277, — 8, *for* tenure *read* tenour.
285, — 30, *for* vigorous *read* rigorous.
300, — 29, *for* ore *read* one.
309, foot-note, line 2, *omit* he.
310, foot-note, line 8, *for* Gujenti *read* Girgenti.
326, line 7, *for* produces *read* produce.
—-, second foot-note, line 2, *for* page 4, 44, *read* 235.
331, line 27, *after* proportions *add* of.
343, — 21, *after* oxygen *insert* a semicolon.
344, — 22, *for* oxymuriatic *read* oxymuriate.
376, — 5, *for* its *read* the volume.
385, — 1, *for* an *read* and.
451, — 17, *for* 128 *read* 1·28.

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